THE

AMERICAN

JOURNAL OF SCIENCE AND ARTS.

[SECOND SERIES.]

ART. I.—*Some Principles of Animal Psychology;* by D. F. WEINLAND.

Read before the American Association, at the Baltimore Meeting, May, 1858.

THE true difference between plants and animals consists in this, that animals have a consciousness of an outer world, while plants have none.

We are accustomed to distinguish animals from plants by their being endowed with free, that is, voluntary locomotion, and with feeling. Linnæus long since said: Saxa crescunt, Vegetabilia crescunt et vivunt, Animalia crescunt vivunt et sentiunt. A certain kind of feeling however cannot be denied to some plants, for instance to the Mimosa. As to the "voluntary" motion, which modern handbooks generally consider as the standard difference between plants and animals, I shall try to show that the term voluntary is far better replaced by the term "conscious of an outer world." What is it that strikes the microscopist as vegetable-nature in the Navicula, and as animal-nature in the Monas? Both move, but the Navicula in its steady onward course runs foul of every obstacle that crosses its way, while the Monas dodging with ease and dexterity, finds its winding way through a host of obstacles, apparently without touching one. It is this evident consciousness of surrounding objects that characterizes the animal.

The consciousness of an outer world is the fundamental principle of the soul of animals. The consciousness of self, of the Ego,

which is rather obscure, even in the highest animals, as it is also in the human child, is proportionate to the consciousness of an outer world; it is a result of the latter, for it is only in opposition to an outer world, that the animal conceives itself and becomes conscious of itself. The degree of psychical development in different kinds of animals may be judged from the degree of development of the consciousness of an outer world. The soul of an animal is the higher, the more relations it has to the outer world, that is, the larger the horizon of its outer world. The latter point I will explain with some illustrations taken from the lower animals, in which the psychical life is more simple, and therefore easier to understand.

What is the outer world of a coral-polyp? With hundreds of its kind it lives on the same coral-stock; it is there fixed and is able to move its mouth and tentacles only; thus it awaits its prey, a little craw-fish, without eyes, and without touching it—by a sense unknown to us—it perceives the presence of its prey, throws out its lasso-cells and catches it. Every individual has both the sexes united. Though closely crowded together, I never could notice a trace of psychical relation between the polyps of the same stock. What is the outer world of such a polyp? The whole range of its psychical life is evidently confined to the objects of food.

Let us now rise one step higher, to a Helminth, an Ascaris, that inhabits the intestine of some vertebrate. In regard to feeding it stands evidently on the same, perhaps on a lower level than the polyp, but still we must rank it psychically higher! The sexes are divided, and in the line of reproduction the male and the female individuals meet each other. There is therefore besides a consciousness of an outer world in regard to food, evidently also, a consciousness of other living individuals, although that consciousness may be dark enough.

We may take a bee, a wasp, or any of the social Hymenoptera, as a third step. In the bee the consciousness of the existence and the interest in other living individuals is not confined merely to the season and to the instinct of reproduction, but to the whole life. At any time the individuals of the bee-hive know each other, give each other signs, help each other, fight for each other. It is evident how much more varied the relations to the outer world, how much more extensive the latter is for a bee than for an Ascaris, and still more than for a polyp.

In order to judge how extensive the outer world is, of which an animal is conscious, that is, in order to judge about its psychical horizon, we must investigate the organs of that consciousness, that is, the psychical organs of animals.

The psychical organs of animals are of three kinds: (1.) RECEPTIVE *organs, organs which receive impressions from the outer*

world; here belongs the whole skin-system including the senses. (2.) REFLECTIVE *organs, that is, organs which combine the impressions received by the receptive organs; here belongs the central nervous system.* (3.) REACTIVE *organs, that is, organs which react upon the outer world; they are the servants of the central nervous system, which go from within outward, while the receptive organs go from without inward. These reactive organs consist in the whole system of voluntary muscles, with the bones which belong to them.*

The student of animal psychology has mainly to depend upon the third kind of organs, namely, the reactive, not only because the functions of the receptive and reflective organs are more or less hidden, but also because their functions are in fact the mirror of the whole psychical life of the animal, being also the resultants of the functions of the receptive and reflective organs.

The functions of the reactive organs are the voluntary motions. When observing these motions in an animal more closely, we soon perceive two kinds of motions, which are in their ends entirely different.

Let us look at a dog. We see in the first place, that it makes many motions, which have no other purpose than to satisfy the Ego of the dog itself. Such are the motions by which it eats, drinks, etc. These motions we call subjective, as having reference exclusively to the Ego, to the subject of the dog itself. But besides these, we see other motions in the dog, which have no immediate reference to the Ego of the dog, but to other dogs, or to men; we see motions of the head, the eyelids, the tail, of the whole body, by which the dog would show to other dogs or to his master, what it thinks, feels or wants. This second kind of motions I propose to call sympathetic motions.

The subjective motions are common to all animals and must be so. We have seen them in the polyp, and we see them in man. They are, generally speaking, the same throughout the animal kingdom. But the greatest diversity exists in regard to the sympathetic motions with different animals, and it will be evident from the following illustration, that the degree of their development is the principal standard for the student of animal psychology. The more the organs for sympathetic motions are developed, the more extensive is the outer world of which the animal is conscious, and the larger is its psychical horizon. Let us compare a fish, a lizard, a monkey and finally man in regard to the organs for sympathetic motions. The fish lying horizontally in the water, its head, neck, trunk and tail forming one continuous massy body; its eyes cold and stiff, turned sidewards, nearly immovable; no voice; hardly traces of an ear,—what organs has this animal to show to its fellow-creatures the processes of its soul? How different a spectacle offers a lizard to the thinking observer! Its body raised upon four legs; a dis-

tinct neck, upon which the head plays freely, thus giving at once to the eyes a horizon not only towards the sides but also upwards and downwards. And how expressive are those eyes! their expression mainly lies in the play of the eyelids, (of which the fish is destitute,) so that from the eyelids alone an experienced observer will perceive, whether the lizard is contented, or sad, or enraged. The tongue, which in the fish is a mere organ for swallowing food, is in the lizard a true organ of sympathetic motions, for we often can see them licking at each other in play or in love. The ear is well developed; they like music and some of them have a voice, as those well know who have spent a night in a virgin forest of the tropics. I will not dwell upon the intermediate degrees of psychical organization as exhibited in birds and the lower mammalia, but consider next the monkey. How rich at once the organization for sympathetic motions. The front legs—in the lizard mere locomotory organs—are in the monkey arms with which the mother embraces the young. The foot, a mere organ of support in the lizard, has become a hand, with which he grasps the hand of his mate. The lips, of which there is no trace in the lizard, are in the monkey very perfect organs of sympathetic motions. With the lips and the whole play of the muscles of the face, with the eyelids, with the tongue, with sounds, etc., the monkey shows to his fellow creatures what it likes and what it hates, what it wants and what it thinks.

Finally let us consider man. The natural position of the monkey is on four legs; in consequence, his head is naturally half bent downwards, thus confining the horizon of his eyes, and his front legs though used as arms are at the same time still organs of locomotion, mainly of climbing. On the contrary, man standing upright on his legs has his arms and hands free, they are perfect organs of sympathetic motions, locomotion being confined to the lower extremities. His head stands free upon the neck, thus giving to all the senses and particularly to the eyes the largest possible horizon. His eye is the mirror of his soul in which the fellow-man reads the innermost thoughts and feelings. His lips, tongue, and the whole apparatus of the larynx produce by their motion the most perfect of all sympathetic motions, language. These and many more not less interesting points are suggested by a comparison of the organs for sympathetic motions, and from the facts principles of practical importance for educational purposes may be derived; but what I have mentioned is sufficient illustration of the truth that owing to the great perfection of organs for sympathetic motions, the relations of man to his fellow beings are far the most diversified and at the same time the most intimate, not only to his fellow beings but to the outer world generally. Whatever our civilization has performed, has been done by improving these natural pyschical

organs. The outer world of the polyp is confined to the objects of its prey, the outer world of the civilized man is the Universe. Our steam vehicles on land and on sea, what else are they than improved organs of locomotion; our letters, our books, our journals, our telegraphs, what else are they than organs of human language on a more extended scale? our telescopes, our microscopes, what else are they than the receptive sense of the eye extended. Thus all the inventions of our civilization tend to enlarge the horizon of the individual man. And this is the true destiny of man. I do not know of a greater motto or life-principle than that which was written on the temple of the oracle of Delphi in ancient Greece: *Γνῶθι σεαυτὸν*—"Know thyself;" but another is equally great, written by Wilhelm von Humboldt, the great philologist, (brother of the author of the Cosmos), it is this: "I wish to leave when dying as little as possible behind me in this world, with what I have not come in contact," that is what I have not mastered with my mind. Humboldt wanted the most perfect knowledge of the outer world, while the Greek philosopher wanted the deepest knowledge of himself. One of these sentiments is only the reverse of the other, or rather it follows immediately from the other. The most thorough knowledge of the outside world involves the deepest insight into ourselves; just as in morals, he who loves his neighbor the truest is the happiest, and thus loves himself the truest.

Art. II.—*On some unusual modes of Gestation;* by Jeffries Wyman, M.D.

Communicated to the Boston Soc. of Natural History. (See Proceedings of the Society, Sept. 15, 1857.)

Among Batrachians the circumstances under which the young are developed, though less varied than in some of the other classes of vertebrates, still present a considerable range. By most species the eggs are deposited in the water either upon aquatic plants or on the bottoms; by others, as in *Salamandra erythronota*, they are laid in damp places under logs or stones; with some the evolution of the embryo commences a short time previous to the laying of the egg and is completed subsequently, while there are other species which are wholly viviparous.

The most remarkable deviations from the ordinary modes are to be found in those instances in which the eggs, after being laid, are again brought into a more or less intimate relation with the parent, as in the "Swamp toads" (*Pipa Americana*) of Guiana, where each ovum is developed in a sac by itself on the back of the female, in *Notodelphys* of Venezuela, where all the eggs are lodged in one large sac, also on the back, and is analogous to the

pouch of the Marsupials, and in *Alytes*, the "Obstetric toad" of Europe, where the eggs are wound in strings around the legs of the male who takes care of them until they hatch.

The species, the habits of which are noticed below, and which, in so far as I have been able to learn, have not attracted the attention of naturalists, adds another to the series just mentioned, though the relation of the fœtus to the parent becomes less intimate than in any of the preceding cases.

Hylodes lineatus (Dum. and Bib.) is very common in Dutch Guiana, and its peculiar habits are well known to the colonists. The first specimen with young which came to my notice had been preserved in alcohol, and was presented to me by Mr. G. O. Wacker, residing at Osembo, on the Para Creek, Surinam, and had been captured at some distance from the water. The young, ten or twelve in number, though separated from the parent, he assured me, when found, were attached to her back.

In the month of May, 1857, during an excursion to the country inhabited by the Bush negroes, above Sara Creek on the upper Surinam River, I had an opportunity for the first time of seeing these animals carrying their young. The grass and bushes were quite wet from a recent fall of rain, and this seemed the inducement that led them from their hiding places, for when the ground was dry none had been seen. They were very quick in their movements, and when alarmed went at once into the grass and thick bushes. One of my companions, Mr. John Green, and myself succeeded in capturing some specimens, which, as we were just leaving the village, were placed at once in alcohol. In one instance the larvæ were retained permanently adherent to the back of the parent, in consequence of the coagulation of the mucus covering the surface of the body, and are still preserved in the Museum of Comparative Anatomy at Cambridge. (Fig. 1.) The young, from twelve to twenty in number, were collected upon the back of the mother, their heads directed towards the middle line. They were about three-fourths of an inch in length. No limbs were developed, though in some of them the rudiments of a leg existed in the form of a small papilla on either side of the base of the tail. No especial organ was found to aid them in adhering to the back of the parent. The adhesion may have been effected by the mouth; this is rendered probable by the fact that all of them had the mouth in contact either with the skin of the parent or with that of another larva. A viscid mucus covering the integuments undoubtedly assisted in some measure to bring about the same results. However this may be, they retained their places perfectly well, and were not displaced when the mother, closely pursued, carried them through the grass.

On dissection of the young nothing was found materially different to conditions of the larvæ of other Anoura. The external

gills had disappeared, but were replaced by internal ones which were arranged as usual on three hyoid arches. The development of the lungs had commenced and these were represented by a slender conical mass of cells, but not permeable to air. The mouth was provided with finely denticulated horny jaws, and the intestinal canal was shorter and less spirally convoluted than in ordinary larvæ of frogs and toads. The stomach was not so much developed as to be distinguished from the rest of the intestine; but this last, after passing the liver, was somewhat dilated, and contained, as was shown by the microscope, large quantities of yolk cells which had not been absorbed and which were adherent to its walls.

We have here then a larva, in all of the details of its structure, especially in the existence of gills and of a flattened tail, adapted to aquatic locomotion and respiration, yet passing a portion of its time at least on the back of its parent and at a distance from the water.

I was not able to ascertain whether the eggs were primarily deposited in the water or not, but it is well known to some of the colonists that after the larvæ have reached a certain degree of development they are carried about in the manner just described and they do not know them under any other circumstances. The existence of yolk cells in the intestine, shows that for a period at least they may have from these a supply of nutriment. But after this is exhausted, and it appeared to be nearly so in those which I have dissected, how do they obtain their food? In the absence of limbs adapted to terrestrial locomotion can they leave the body of the parent? and if they cannot, do they, as in the case of Pipa and probably in Notodelphys, depend upon a secretion from her?

Among Fishes, as far as at present known, the external conditions under which the eggs are developed are more varied than in any other class of Vertebrates. There are scarce any known conditions of the higher classes to which there are not analogies at least in the class of fishes. Besides the ordinary mode of depositing eggs upon the bottoms, some of the Salmonidæ, like the turtles, bury their eggs, the Lampreys (*Petromyzon*), the Breams, (*Pomotis*), the Hassars (*Callicthys*), the Stickle-backs (*Gasterostei*), &c., build more or less complete nests. Among some of the Pipe fishes, (*Syngnathidæ*), the eggs and subsequently the young, are carried in a pouch analogous to that of the opossums and other marsupial animals, and among some of the Sharks there is a vitelline placenta analogous to the Allantoidian one of the Mammalia.*

* Prof. Owen (in Philos. Transactions, 1834.) has pointed out the vascular relations of the fœtal Kangaroo to the parent. The chorion is not vascular, but the umbilical vesicle is largely provided with blood vessels, and, as far as his investigations go,

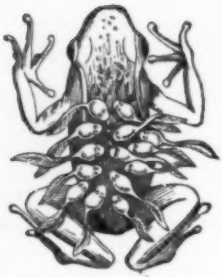


Fig. 1.—Hylodes lineatus.

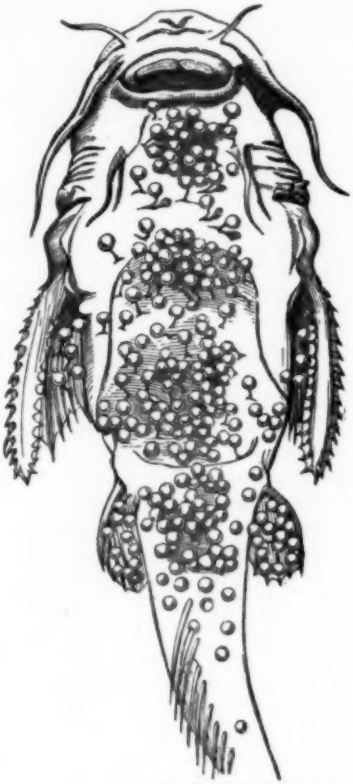


Fig. 2.—Aspredo lævis.

Fig. 4.—Pedicle showing capillary plexus, enlarged ten diameters.

Fig. 3.—Pedicle with an egg attached, enlarged 4 diameters.

To those species enumerated above where the eggs become more or less intimately connected with the body of the parent after they are laid may be added the Aspredos and some species of Bagrus, from Guiana.

Aspredo lævis (Cuv. and Val.), the "Trompetti" of the colonists, is about fifteen inches in length, and belongs to a remarkable genus of Siluroid fishes, which, in addition to several peculiarities of anatomical structure, are remarkable for carrying the eggs and young attached to the under surface of the body. These fishes are very abundant in the waters of the Surinam where they are taken in the nets with other kinds. They are not used as articles of food except by the negroes, who have a fancy for Siluroids generally, and in consequence these are known among the colonists as *Ningré fisi* or "nigger fish." A general account of the internal structure of Aspredo, is given in the Hist. Nat. des Poissons, by Cuvier and Valenciennes, T. xv, p. 35.

In describing the organs of reproduction, Valenciennes says: "the ovaries are small and contain very large eggs, which leads to the belief that this fish is viviparous." In those specimens which I have dissected the eggs when mature are not remarkable for their very great size, being from 0·09 to 0·11 inch in diameter, even after the commencement of the development of the fœtus, and when the egg has already increased in size. The ovaries are about an inch and a half long and completely separated from each other.

Valenciennes further describes certain appendages to the under side of the body: "A certain number of individuals in each species (of Aspredo) are remarkable for singular appendages on the under side of the thorax and abdomen, and which, after the few observations which I have been able to make appear to indicate a certain state of the female. I have not seen them in the males and the females do not have them at all times. They first appear as pores on the under and naked surface of the trunk; and these enlarge and swell into tubercles, which subsequently elongate into filaments, and the extremity of each filament is dilated into a small cupule." *

"It was in this state that Bloch saw them in an individual with six cirrhi, and, taking them for specific characters, named the fish *Platystacus cotylophorus*. But I have seen the same appendages in three species. Artedi, in the text of Seba, had

affords the principal vascular surface by means of which an interchange takes place between the fœtus and the parent. The vitelline circulation then, as in sharks, is the respiratory circulation. The allantois of the Marsupials appears to remain in a rudimentary condition, and does not form a connection with the parent. Thus the vascular relation of the fœtus of some of the sharks, as Carcharias, with the parent is identical with that of the Marsupials.

* Cuvier and Valenciennes, Hist. Nat. des Poissons. T. xv, p. 430.

already described two species, to which we now add a third. All three live in the waters of Guiana and this is all we know of their habits."*

From the preceding paragraphs it does not appear that Valenciennes had supposed that the so-called "cupules" were intended to contain or had contained ova, especially as he had previously expressed the belief that the Aspredos, in consequence of the large size of the eggs, were viviparous. The true use of the appendages in question relates to the development of the eggs, as the following description will show. The habits of the fish are well known to the fishermen, from one of whom Mr. Green obtained information with regard to their peculiar mode of gestation. After many ineffectual efforts, we at last succeeded in procuring the specimens on which the following observations were made, and Mr. Green has kindly presented to me some very fine ones from his own collection, without which this notice would have been much less complete.†

In the month of June the eggs are found adhering to the underside of the body, to the ventral and pectoral fins, and extend as far forward as the under lip, and as far backwards as the middle of the tail. (Fig. 2.) In some, however, the distribution is much more limited. I was unable to learn anything with regard to the transfer of the ova from the genital orifice to the point of their attachment. The only organ which seems in any way adapted to such a purpose is the slender and flexible tail terminated by a delicate caudal fin. It is possible that the eggs may be deposited on the bottom of the river, and subsequently attached by pressing the under side of the body upon them.

In those individuals where the ova were still in the ovary, but approaching maturity, the integuments of the under side of the body gave no other indications of the changes about to take place than of being quite vascular; the skin was perfectly smooth, no "pores" were visible, but a large vessel was seen emerging from the region of the liver, and descending along the median line gave off branches quite freely to the integuments. This may have some relation to the future development of the pedicles which support the eggs and perhaps to the nutrition of the embryo as will be adverted to hereafter.

In all the specimens which I have had an opportunity of examining, the eggs were either somewhat advanced or quite mature; so that no observations could be made on the earlier conditions of the egg and the formation of its pedicle. The pedicle is a flexible outgrowth from the common integuments, is about two lines in length, is attached to the skin by a slightly expanded base, and spreads out at its summit into a shallow cup

* Cuvier and Valenciennes, Hist. Nat. des Poissons. T. xv, p. 430.

† See an account of the habits of the Aspredo by Mr. Green in the Proceedings of the Boston Soc. of Nat. History for April, 1858.

or "cupule," for the support of the egg. It is composed almost entirely of fibrous tissue, invested with a layer of tesselated epithelium. In some instances when the eggs were but little advanced, numerous fusiform cells were detected among the fibres. It is vascular, two or three vessels reaching to the cup, where they ramify and form a somewhat extended capillary plexus. (Figs. 3 and 4).

The eggs vary according to the degree of development from the 0·09 to 0·15 of an inch in diameter, and are covered with an external homogeneous membrane, containing minute punctiform depressions—within this is a second, of a brownish color and composed of epithelium. The embryos which were the most advanced and just ready to hatch, had not as yet completely absorbed the yolk, and were coiled up within the membranes, which in consequence of the irregularities of the mass formed by the embryo, had no longer a spherical form.

The eggs are retained in connection with the cup apparently by adhesion alone, for as soon as the fœtus escapes, the egg membranes become very easily detached from the pedicle, and this last as shown by some of the specimens undergoes absorption.

The relation of the embryo to the parent in this singular mode of gestation cannot be determined very accurately, but the vascular plexus in the cup, seems to be more than is necessary for the mere nutrition of the part. The egg increases in size during incubation, those ova in which development had but slightly advanced measuring from 0·09 to 0·11 of an inch in diameter, while those nearly mature measured from 0·14 to 0·15 of an inch. How this increase of size of the embryo over the original size of the egg is actually obtained I have no facts to show, but either of two suppositions are probable; it may be by absorption of materials from the water which surrounds it, or from the capillary plexus of the pedicles, and in this case in a manner analogous to that of Pipa.

Among the Siluroid fishes of Guiana there are several species, which at certain seasons of the year have their mouths and branchial cavities filled either with eggs or young, and as is believed for the purpose of incubation. My attention was first called to this singular habit by the late Dr. Francis W. Cragin, formerly U. S. Consul at Paramaribo, Surinam. In a letter dated August, 1854, he says, "the eggs you will receive are from another fish. The different fishermen have repeatedly assured me, that these eggs in their nearly mature state are carried in the mouths of the parent, till the young are relieved by the bursting of the sac. Do you either know or believe this to be so, and if possible, where are the eggs conceived and how do they get into the mouth?"

In the month of April, 1857, on visiting the market of Paramaribo, I found that this statement, which at first seemed to be very improbable, was correct as to the existence of eggs in the mouths of several species of fish. In a tray of fish which a negro woman offered for sale, I found the mouths of several filled with either eggs or young, and subsequently an abundance of opportunities occurred for repeating the observation. The kinds most commonly known to the colonists, especially to the negroes, are *Jara-bakka*, *Njinge-njinge*, *Koepra*, *Makrede* and one or two others, all belonging either to the genus Bagrus or one nearly allied to it. The first two are quite common in the market and I have seen many specimens of them; for the last two I have the authority of negro fishermen but have never seen them myself. The eggs in my collection are of three different sizes, indicating so many species; one of the three having been brought to me without the fish from which they were taken.

The eggs become quite large before they leave the ovaries, and are arranged in three zones corresponding to three successive broods, and probably to be discharged in three successive years; the mature eggs of a Jara-bakka eighteen inches long, measure three fourths of an inch in diameter, those of the second zone one fourth; and those of the third or very minute, about one sixteenth of an inch.

A careful examination of eight specimens of Njinge-njinge about nine inches long, gave the following results:

The eggs in all instances were carried in the mouths of the males. This protection, or gestation of the eggs by the males, corresponds with what has been long noticed with regard to other fishes, as for example, Syngnathus where the marsupial pouch for the eggs or young is found in the males only, and Gasterosteus where the male constructs the nest and protects the eggs during incubation, from the voracity of the females.

In some individuals the eggs had been recently laid, in others they were hatched, and the fœtus had grown at the expense of some other food than that derived from the yolk, as this last was not proportionally diminished in size, and the fœtus weighed more than the undeveloped egg. The number of eggs contained in the mouth was between twenty and thirty. The mouth and branchial cavity were very much distended, rounding out and distorting the whole hyoid and branchiostegal region. Some of the eggs even partially protruded from the mouth.

The ova were not bruised or torn as if they had been bitten, or forciby held by the teeth. In many instances the fœtuses were still alive, though the parent had been dead for many hours.

No young or eggs were found in the stomach, although the mouth was crammed to its fullest capacity.

The above observations apply to Njinge-njinge. With regard to Jarra-bakka, I had but few opportunities for dissection, but in several instances the same conditions of the eggs were noticed as stated above; and in one instance, besides some nearly mature fœtuses contained in the mouth, two or three were squeezed apparently from the stomach; but not bearing any marks of violence or of the action of the gastric fluid. It is probable that these found their way into that last cavity after death, in consequence of the relaxation of the sphincter which separates the cavities of the mouth and the stomach. These facts lead to the conclusion that this is a mouth gestation, as the eggs are found there in all stages of development, and even for some time after they are hatched.

The question will be very naturally asked, how under such circumstances, these fishes are able to secure and swallow their food. I have made no observations bearing upon such a question. Unless the food consists of very minute particles, it would seem necessary that during the time of feeding the eggs should be disgorged. If this supposition be correct, it would give a very probable explanation of the only fact which might be considered at variance with the conclusion stated above, viz., that we have in these fishes a mouth gestation. In the mass of eggs with which the mouth is filled, I have occasionally found the eggs, rarely more than one or two, of another species. The only way in which their presence may be accounted for, it seems to me, is by the supposition that while feeding, the eggs are disgorged, and as these fishes are gregarious in their habits, when the ova are recovered, the stray egg of another species may be introduced into the mouth among those which naturally belong there.

5.

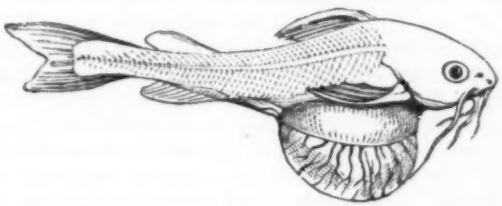


Fig. 5 represents a nearly mature fœtus of the natural size from the mouth of Bagrus, with the yolk sac partially included in the cavity of the abdomen.

ART. III.—*Some Facts respecting the Nitrates;* by JOHN M. ORDWAY.

WHILE studying the nitrates of the sesquioxyds I found it advisable, for the sake of comparison, to examine the protonitrates also, with reference to some points not generally taken into account in enumerating the properties of these salts. And as the nitrates are among the most common and important salts, it may be worth the while to exhibit these gleanings in fields often gone over but not yet entirely cleared. There are few new facts to be brought forward, but the chief object of this paper is to show the fitness of certain means for the illustration of some general truths already well known.

In most chemical text-books no good instances are given of the development of heat by mere solidification. It is indeed usually mentioned that water may be cooled many degrees below the freezing point and remain liquid, and that on congealing its temperature suddenly rises to 32° F. But the experiment is so troublesome to make, especially in the lecture room, that these truths commonly pass as matters of faith rather than of sight, and the important principles which they illustrate, often fail of being distinctly impressed on the mind of the student. Now many of the hydrated salts, and among them the nitrates, melt at points above the common temperature of the air, and are therefore well adapted for showing, at all seasons and with great ease and clearness, the inertia of bodies with regard to change of form and the liberation of sensible heat by crystallization.* Nitrate of lime is preëminently suitable for the exhibition of these properties, since after having been fused and heated above 150° F., it may be cooled in a glass vessel as low as 60°, and kept in the liquid state a long time, often for several days; but on dropping in a bit of the solid nitrate, crystallization immediately commences, and an inserted thermometer soon rises to 110° F.

A substance which may be had both liquid and solid at a temperature considerably below the melting point, is obviously very convenient for displaying the comparative densities and specific heats in the two forms, as complications caused by differences of temperature, may be entirely avoided. Thus the specific gravity of a specimen of nitrate of lime in the liquid state, at 60° F., was found to be 1·79. Some of the same was poured into oil of turpentine, made to solidify, and cooled to

* In an excellent work published in 1857,—"Lehrbuch der physikalischen und theoretischen Chemie, von H. Buff, H. Kopp und F. Zamminer,"—hyposulphite of soda is mentioned as capable of affording a very striking example of the heat becoming free during fixation; but this salt is less easy to prepare than most of the nitrates.

60° F. Its density was now 1·90. The contraction may be rendered appreciable by the eye, if we cool to a certain degree some melted nitrate contained in a long necked flask, fill with an oil up to a marked height, effect the crystallization, and then cool to the same point as before.

To illustrate the absorption of heat during the liquefaction of solids, freezing mixtures are commonly employed in which one of the ingredients, ice, is already cold. The experiment is more striking when all the articles used are at the temperature of the surrounding air. Such may be the case if we take crystallized sulphate of soda and a sesquinitrate. A mixture of 36 grams of powdered pernitrate of iron crystals and 57 grams of fine Glauber's salt, liquefied and lowered thermometer from 65° F. to zero. It readily froze water contained in a test tube. In cold weather, 8 grams of the nitrate and 9·5 grams of the sulphate brought the thermometer from 22° to −10°.*

In manufacturing salts on a large scale, the hydrometer is a very useful and ready instrument for determining when a solution is of the right strength to crystallize. But the quantities operated on in the laboratory are generally so small that the hydrometer can hardly be made available. The bulb of a thermometer, however, requires but little depth of liquor, and hence to one who wishes to prepare in the small way any of the highly soluble salts, a knowledge of the boiling points of the desired products may be of great service. Thus, finding that crystallized protochlorid of tin melts at 107° F., boils at 251°, and may be cooled to 83° without becoming solid, we see that to make this article in midsummer the evaporation of the weak solution must be continued till the boiling point gets nearly or quite up to 251°.

It should be remarked that the melting and boiling points given below, do not pretend to absolute exactness. No two different lots of the same salt are likely to give just the same figures; for it is next to impossible to get most of the hydrated salts exactly dry,—neither effloresced nor retaining mother liquor in the interstices. For any particular specimen the point of fusion can be determined with great precision. But the boiling points are high and, unless very nice precautions are taken, there will be some loss of water in heating up. So to find the temperatures of incipient ebullition, crystals were taken that were not entirely dry to start with, and the correctness of the indications was judged of after ascertaining the solidifying points of the residues.

There is, of course, no definite limit to the cooling which a melted salt may undergo without beginning to crystallize. I

* Nitrate of iron is pretty corrosive, and should not be touched with the fingers.

have here put down the lowest point at which each of the salts tried has actually been observed to remain liquid without any special precautions being taken to retard the crystallization. With some few of the nitrates it makes a difference in this respect whether they are moderately or strongly heated. If any one of them is heated but little more than is necessary to effect its fusion, it will begin to solidify before it gets many degrees below the melting point. But when the nitrates of lime, iron and chrome are brought nearly up to the boiling point, they can be cooled very low before they begin to shoot. Considering the difference in color between the solid and the liquid nitrates of iron, chrome and cobalt, there seems to be no special absurdity in supposing that some strongly heated nitrates may have to overcome a reluctance to change of state as well as one to change of form, and are therefore slower in beginning to show crystals.

The composition has in each instance been determined anew, either by simple ignition or by drying down with an excess of sulphuric acid, and so there is little room for error. It appears that a greater uniformity prevails among the nitrates than among any other salts. In all but four of those examined there are either six or three equivalents of water to each equivalent of nitric acid. In only two cases, has the same base been found capable of forming two different crystallized nitrates.

SEXHYDRATES.

Nitrate of Magnesia, $\dot{M}g\,\dot{N}\,\dot{H}_6$.—This salt melts at 194° F. The liquid has been cooled to 188°. It boils at 290°.

When the heating is continued, the salt remains liquid and clear till about five equivalents of water and a little of the acid are expelled. The residue is not entirely soluble. It becomes hot in recombining with water.

Nitrate of Zinc, $\dot{Z}n\,\dot{N}\,\dot{H}_6$.—Melts at 97½° F. It has been cooled in the liquid form to 87°. It boils at 268°.

Some of the melted crystals, on continued boiling, remained thin and clear till 42 p. c. of the weight was gone. The residue hardened to a vitreous mass on cooling, which had a composition not far from $\dot{Z}n_4\,\dot{N}_3\,\dot{H}_3$. This substance did not heat much when treated with water; but when some crystals were boiled till about four equivalents of water passed off, the residue evolved considerable heat in recombining with water.

Nitrate of zinc cannot be heated long without becoming basic and partially insoluble in water.

Nitrate of Manganese, $\dot{M}n\,\dot{N}\,\dot{H}_6$.—Melts at 78½° F. Some dry crystals liquefied in a stoppered bottle during the hot weather of June and remained melted till September, though the temperature was sometimes as low as 60°. It boils at 265°.

If the boiling is continued, decomposition soon commences and black oxyd of manganese is precipitated. This gradual formation of peroxyd is also effected by a long continued steam heat.

Some liquid nitrate at 70° F. was found to have a density of 1·8104, while the solid salt at 70° had the specific gravity 1·8199.

Nitrate of Nickel, $\dot{N}i.\ddot{N}.\dot{H}_6$.—Melts at 134° F. The liquid has been cooled to 115°. It boils at 278°.

When the boiling is continued the liquid remains clear till three equivalents of water are expelled. It then begins to thicken and parts with acid.

Nitrate of Cobalt, $\dot{C}o.\ddot{N}.\dot{H}_6$.—I had too little of this to determine accurately the melting and boiling points, but they differ little from those of the nickel salt.

Pernitrate of Iron, $\dot{F}e + 3\ddot{N}\dot{H}_6$.—Melts at 117° F. May remain liquid at 70°, after being strongly heated. It boils at 257° F.

The specific gravity of some in the liquid state at 70° F., was found to be 1·6712, while the same solidified and cooled to 70°, had a density of 1·6835.

Nitrate of Chrome, $\dot{C}r + 3\ddot{N}\dot{H}_6$.—This salt melts at about 98° F. It has been cooled to 68°. It boils at 258°.

Nitrate of Alumina, $\dot{A}l + 3\ddot{N}\dot{H}_6$.—Melts at 163° F., can be cooled to 147½°, and boils at 273°.

Nitrate of Uranium, $\ddot{U}\ddot{N}\dot{H}_6$.—This beautiful salt melts at 139°. It may remain liquid at 115°. It begins to boil at 245°.

When the boiling was continued, the stuff remained thin and clear till about four equivalents of water and a little of the acid passed off. The residue gave with water a solution which was turbid at first but soon became clear. Some heat was evolved during the solution.

Nitrate of Copper.—When nitrate of copper crystallizes at a low temperature, it forms a pale blue salt having the composition $\dot{C}u\ddot{N}\dot{H}_6$. These crystals are not permanent in hot weather, for at 79½° F. they break up into a liquid and crystals of the trihydrate. To make the whole liquid requires a heat above 100° F., and so the pale crystals have no definite solidifying point.*

TRIHYDRATES.

Nitrate of Copper, $\dot{C}u\ddot{N}\dot{H}_3$.—This is the formula of the crystals which form above 79½° F. They have nearly the same shape as the sexhydrate sometimes assumes, but are deep blue and are permanent in every state of the air. The composition is erroneously given in some books as $\dot{C}u\ddot{N}\dot{H}_4$,—probably because the analysts took no pains to ascertain the dividing limit between the two salts, and tried a mixture.

* A solution of nitrate of copper is sometimes sold, standing at 55° B. As a solution saturated at 50° F. has just this strength, it is not strange that the maker often finds his returned carboys broken by huge masses of pale crystals.

The trihydrate melts at 238°. It has been cooled down to 224° before beginning to shoot. It boils at 338°.

If the boiling is continued, nitric acid immediately begins to pass off, and a green basic nitrate is deposited.

Nitrate of Lanthanum, $La\dot{N}\dot{H}_3$.—This was found to melt at 104° F., and was cooled to 70° without crystallizing immediately. It boiled at 258°. These figures, however, cannot be considered as exact, for the salt used for trial amounted to but 32 grams, and was not absolutely free from didymium and cerium.

Nitrate of Glucina, $\dot{B}e + 3\dot{N}\dot{H}_3$.—Melts at 140° F. and may be cooled as low as 85° before it begins to fix. It boils at 285°.

Some boiled till the thermometer rose to 320°, gave off acid, but remained perfectly clear. When this residue was cooled to 61°, a crystal did not cause it to solidify, because it was too basic. But the addition of strong nitric acid, induced a rapid crystallization, the temperature rising to 142°.

When the salt was boiled not quite so long, the product could be made to solidify, but the resulting temperature was considerably lower. Dilution with a basic salt, has therefore the same effect on the melting point as dilution with water.

TETRAHYDRATES.

Nitrate of Strontia, $\dot{S}r\dot{N}\dot{H}_4$.—Unlike any other hydrated nitrate, this salt crystallizes in the monometric system.

The composition of hydrated nitrate of strontia is always laid down in the books as $\dot{S}r\dot{N}\dot{H}_5$. But this formula has no analogy in its favor, and having repeatedly tried good crystals formed at a low temperature I have invariably found but four equivalents of water. The nitrate crystallized above 75° F., is generally anhydrous, and that formed below 60° is hydrated, but between these temperatures there is no certainty. Thus a solution saturated at 84° F., while cooling down to 62°, deposited nothing but anhydrous crystals; and a solution saturated at 71°, by standing some hours where the thermometer did not get below 70°, gave only fully hydrated crystals.

The hydrated salt is resolved by heat into a liquid and the anhydrous nitrate. Even the hot weather of summer causes it to sweat, if kept in a close vessel. In dry air, it loses all its water by efflorescence.

Nitrate of Lime, $\dot{C}a\dot{N}\dot{H}_4$.—Melts at 111° F. Some that was heated only to 124°, began to crystallize when it had cooled to 96°. After being heated to 153°, it remained liquid over night and got down to 57½°. This salt boils at 270°. When the boiling is continued, the mass remains liquid and clear till about one third of the water passes off. Farther heating renders it anhydrous, with scarcely any loss of acid. This dry residue evolves a strong heat in recombining with water.

Nitrate of Cadmium, $\dot{C}d\,\dot{N}\,\dot{H}_4$.—This salt melts at 139° F. It has been cooled to 91° before beginning to crystallize. It boils at about 270°. On continued boiling it continues clear and thin till nearly three equivalents of water are gone. When all the water has passed off, a small portion of the remaining dry mass is insoluble.

Nitrate of Bismuth, which was formerly supposed to be a trihydrate, has been found more recently to have the anomalous composition $\dot{B}i\,\dot{N}_3\,\dot{H}_{10}$. In several trials of a pure nitrate dried over sulphuric acid, I have obtained, by ignition, 48 per cent of oxyd. This would make the quantity of water as near eleven as ten equivalents. It is barely possible then that nitrate of bismuth may be a combination of a trihydrate with a tetrahydrate and have for its true formula $\dot{B}i\,\dot{N}_3\,\dot{H}_9 + \dot{B}i\,\dot{N}_3\,\dot{H}_{12}$.

This salt is not deliquescent and does not effloresce, even when kept for a long time over sulphuric acid. It is insoluble, as a whole, in water, and does not melt clear. At 163½° F., it resolves itself into a clear liquid and one opaque solid. The mixture has been cooled to 155°, but on stirring it solidified again while the temperature rose to 163½°. Some of the liquid part decanted clear, formed on cooling a mass of crystals quite wet with acid and having altogether a composition not far from $\dot{B}i_3\,\dot{N}_{11}\,\dot{H}_{38} = 3\,\dot{B}i\,\dot{N}_3\,\dot{H}_{10} + 2\,\dot{N}\,\dot{H}_4$.

ART. IV.—*Further Observations on the Allotropic Modifications of Oxygen, and on the Compound Nature of Chlorine, Bromine, &c.;* by Professor SCHÖNBEIN.*

THESE last six months I have been rather busily working on oxygen, and flatter myself not to have quite in vain maltreated my favorite; for I think I can now prove the correctness of that old idea of mine, according to which there are two kinds or allotropic modifications of active oxygen, standing to each other in the relation of + to −, *i. e.* that there is a positively-active and a negatively-active oxygen,—an ozone and an ant-ozone, which on being brought together neutralize each other into common or inactive oxygen according to the equation $(+\dot{O}) + (-\dot{O}) = O$.

The space allotted to a letter being so small, I cannot enter into the details of my late researches, and must confine myself to some general statements, which I hope, however, will give a clear notion of the nature of my recent doings. A paper will

* Addressed as a letter to Prof. Faraday, and communicated by him to the L., E. and D. Phil. Mag., xvi, 178.

before long be published in the Transactions of the Academy of Munich.

Ozonized oxygen, as produced from common oxygen by the electrical spark or phosphorus, is identical with that contained in a number of oxy-compounds, the principal ones of which are the oxyds of the precious metals, the peroxyds of manganese, lead, cobalt, nickel and bismuth,—permanganic, chromic and vanadic acids; and even the peroxyds of iron and copper may be numbered among them.

The whole of the oxygen of the oxyds of the precious metals exists in the ozonic state, whilst in the rest of the oxy-compounds named, only part of their oxygen is in that condition. I call that oxygen negatively-active, or ozone *par excellence*, and give it the sign $-\mathring{O}$ on account of its electromotive bearing. Though generally disinclined to coin new terms, I think it convenient to denominate the whole class of the oxy-compounds containing $-\mathring{O}$ "ozonids." There is another less numerous series of oxy-compounds in which part of their oxygen exists in an opposite active state, *i. e.* as $+\mathring{O}$ or antozone, wherefore I have christened them "antozonids." This class is composed of the peroxyds of hydrogen, barium, strontium, and the rest of the alkaline metals; and on this occasion I must not omit to add, that what I have hitherto called ozonized oil of turpentine, ether, &c., contain their active oxygen in the $+\mathring{O}$ state, and belong therefore to the class of the "antozonids."

Now, on bringing together (under proper circumstances) any ozonid with any antozonid, reciprocal catalysis results, the $-\mathring{O}$ of the one and the $+\mathring{O}$ of the other neutralizing each other into O, which, as such, cannot be retained by the substances with which it had been previously associated in the $-\mathring{O}$ or $+\mathring{O}$ condition. The proximate cause of the mutual catalysis of so many oxy-compounds depends therefore upon the opposite states of the active oxygen contained in those compounds.

I will now give some details on the subject.

1. Free ozonized oxygen $=(-\mathring{O})$, and peroxyd of hydrogen $=HO+(+\mathring{O})$, or peroxyd of barium $=BaO+(+\mathring{O})$ (the latter suspended in water), on being shaken together destroy each other, $HO+(+\mathring{O})$ or $BaO+(+\mathring{O})$ being reduced to HO or BaO, and $+\mathring{O}$ and $-\mathring{O}$ transformed into O.

2. Aqueous permanganic acid $=Mn^2O^2+5(-\mathring{O})$, or a solution of permanganate of potash mixed with some dilute nitric acid is almost instantaneously discolored by peroxyd of hydrogen or

peroxyd of barium, the nitrate of the protoxyd of manganese being formed in the first case, and in the second, besides this salt, the nitrate of baryta. It is hardly necessary to state, that in bothe ases the $-\mathring{O}$ of the permanganic acid and the $+\mathring{O}$ of the peroxyd of hydrogen or barium are disengaged as O.

3. An aqueous solution of chromic acid containing some nitric or sulphuric acid and peroxyd of hydrogen are rapidly transformed into the nitrate or sulphate of oxyd of chromium, HO, and inactive oxygen, which is of course disengaged. A solution of chromic acid mixed with some nitric acid and BaO^2 gives a similar result, nitrate of baryta and oxyd of chromium being formed, and O disengaged.

4. If you add to a mixture of any peroxyd salt of iron and the red ferro-sesquicyanuret of potassium (both substances dissolved in water) some peroxyd of hydrogen, prussian blue will be thrown down and inactive oxygen set free. On introducing into a mixture of nitrate of peroxyd of iron and the ferro-sesquicyanuret of potassium the peroxyd of barium, a similar reaction takes place, prussian blue, nitrate of baryta, &c., being formed, and inactive oxygen eliminated. From these facts it appears that, under certain conditions, even peroxyd of iron and HO^2 or BaO^2 are capable of catalyzing each other into FeO and HO, or BaO and O.

5. Under certain circumstances PbO^2 or MnO^2 are soluble in strong acetic acid; now if you add to such a solution HO^2 or BaO^2, the peroxyds will be reduced to HO or BaO, and PbO or MnO, inactive oxygen being disengaged.

6. It is a well known fact that the oxyd of silver $=Ag(-\mathring{O})$, or the peroxyd of that metal $=Ag(-\mathring{O})^2$, and the peroxyd of hydrogen $=HO+(+\mathring{O})$, catalyze each other into metallic silver, water and inactive oxygen. Other ozonids such as $PbO+(-\mathring{O})$ or $MnO+(-\mathring{O})$, on being brought in contact with $HO+(+\mathring{O})$, are transformed into PbO or MnO, HO and O. Now the peroxyd of barium $=BaO+(+\mathring{O})$, acts like $HO+(+\mathring{O})$. If you pour water upon an intimate mixture of AgO, or AgO^2 and BaO^2, a lively disengagement of inactive oxygen will ensue, AgO, AgO^2 and BaO^2 being reduced to metallic silver and baryta. In concluding the first part of my letter, I must not omit to state the general fact, that the oxygen disengaged in all cases of reciprocal catalysis of oxy-compounds, behaves in every respect like inactive oxygen.

There is another set of chemical phenomena, in my opinion closely connected with the polar states of the active oxygen contained in the two opposite classes of peroxyds. It is known that

a certain number of oxy-compounds, for instance the peroxyds of manganese, lead, nickel, cobalt, bismuth, silver, and also permanganic, chromic, and vanadic acids, furnish with muriatic acid chlorine, whilst another set, such as the peroxyds of barium, strontium, potassium, &c., are not capable of eliminating chlorine either out of the said acid or any other chlorid. This second class of oxy-compounds produces, however, with muriatic acid, the peroxyd of hydrogen; and it is quite impossible in any way to obtain from the first class of the peroxyds HO^2, or from the second chlorine.

You are aware that, from reasons of analogy, I do not believe in the doctrine of chlorine, bromine, &c., being simple bodies, but consider those substances as oxy-compounds, analogous to the peroxyds of manganese, lead, &c., in other terms, as "ozonids." Chlorine is therefore to me the peroxyd of murium $=MuO+(-\mathring{O},)$ hydrochloric acid $=MuO+HO$, and, as already mentioned, the peroxyd of barium $=BaO+(+\mathring{O},)$ that of hydrogen $=HO+(+\mathring{O},)$ and the peroxyd of manganese $=MnO+(-\mathring{O})$. Proceeding from these suppositions, it is very easy to account for the different way in which the two sets of peroxyds are acted upon by muriatic acid.

From reasons as yet entirely unknown to us, HO can be chemically associated only with $+\mathring{O}$, and with no other modification of oxygen, to constitute what is called the peroxyd of hydrogen; and in a similar way MuO (the hypothetically anhydrous muriatic acid of older times) is capable of being united only to $-\mathring{O}$ to form the so-called chlorine, which I denominate peroxyd of murium. If we cause $MuO+HO$ to react upon $BaO+(+\mathring{O},)$ MuO unites with BaO, and HO with $+\mathring{O}$; but if you bring together $MuO+HO$ with $Mn+(-\mathring{O},)$ part of MuO is associated to MnO, another part to $-\mathring{O}$, water being eliminated, according to the equation

$$2(MuO, HO)+MnO+(-\mathring{O})=MuO, MnO+MuO, (-\mathring{O})+2HO.$$

As you will easily perceive, from these views it would follow that, under proper circumstances, two opposite peroxyds, on being intimately mixed together and in the right proportion and acted upon by muriatic acid, could yield neither chlorine nor peroxyd of hydrogen, but merely inactive oxygen. If somewhat dilute muriatic acid be poured upon an intimate mixture of five parts of peroxyd of barium and two parts of peroxyd of manganese, the whole will be rapidly transformed into the muriates of baryta and protoxyd of manganese, the active oxygen of both the peroxyds being disengaged in the inactive condition, and

not a trace of free chlorine making its appearance. The same result is obtained from dilute hydrobromic acid.

Another consequence of my hypothesis is this: that an intimate and correctly proportioned mixture of two opposite peroxyds, such as the peroxyd of barium and that of lead, on being acted upon by any oxy-acid, cannot produce the peroxyd of hydrogen; or, to express the same thing in other terms, muriatic acid must act upon the said mixture exactly in the same way as the oxy-acids do; and that is indeed the case. Mixtures of the peroxyds just mentioned and acetic or nitric acids, are readily converted into the acetates or nitrates of baryta and protoxyd of manganese, the active oxygen of both the peroxyds being of course disengaged in the inactive condition.

Before I close my long story I must mention one fact more, which, in my opinion, is certainly a very curious one. If you mix an aqueous and concentrated solution of bromine with a sufficient quantity of peroxyd of hydrogen, what happens? A very lively disengagement of inactive oxygen takes place, the color and the odor of the bromine solution disappear, the liquid becomes sour, and on adding some aqueous chlorine to it, bromine reappears. From hence we are allowed to conclude, that, on bringing bromine into contact with peroxyd of hydrogen, some so-called hydrobromic acid is produced. The hypothesis at present prevailing cannot account for the formation of that acid otherwise than by admitting that bromine takes up the hydrogen of HO^2, eliminating the two equivalents of oxygen united to H. I, of course, take another view of the case; bromine is to me an ozonid like peroxyd of lead, &c., *i. e.*, the peroxyd of bromium $=BrO+(-\dot{O})$. Now $HO+(+\dot{O})$ and $BrO+(-\dot{O})$ catalyze each other into HO, BrO, and inactive oxygen, BrO+HO forming hydrobromic acid, or what might more properly be called hydrate of bromiatic acid.

It will be perceived that I am growing more and more hardened in my heretical notions, or, to speak more correctly, in my orthodox views; for it was Davy who acted the part of a heretic in overthrowing the old, venerable, true creed. Indeed the longer I compare the new and old doctrine on the nature of chlorine, &c., with the whole material of chemical facts bearing upon them, the less I am able to conceive how Davy could so lightly and slightly handle the heavy weight of analogies which, in my opinion, speak so very strongly and decisively in favor of Berthollet's views. There is no doubt Sir Humphrey was a man of great genius, and consequently very imaginative; but I am almost inclined to believe that, by a certain wantonness, or by dint of that transcendent faculty of his mind, he was seduced to conjure up a theory intended to be as much out of the way and "*invraisemblable*" as possible, and serve nevertheless certain

theoretical purposes; and certainly, if he entertained the intention of solving such a problem, he has wonderfully succeeded. But what I still more wonder at is both the sudden and general success which that far-fetched and strained hypothesis met with, and the tenacity with which the whole chemical world has been sticking to it ever since its imaginative author pleased to divulge it: and all this could happen in spite of the fact that the new doctrine, in removing from the field of chemistry a couple of hypothetical bodies, was, for analogy's sake, forced to introduce fictitious compounds, not by dozens only, but by hundreds,—the oxy-sulphion, oxy-nitrion, and the rest of those "nonentia." But enough of this subject, upon which I am apt to grow warm and even angry. Although the results I have obtained from my recent investigations cannot but induce me to begin another, and, I am afraid, endless series of researches, I shall for the present cut short the matter and indulge for some time in absolute idleness.

Bâsle, June 25th, 1858.

ART. V.—*Occurrence of Cobalt and Nickel in Gaston county, North Carolina;* by HENRY WURTZ, Prof. Chemistry, National Medical College, Washington.

Read in part before the American Association for the Advancement of Science at Baltimore, April, 1858. *

WHILE exploring a large tract of land in Gaston and Lincoln counties, N. C., during the summer of 1857, I found indications of the existence of ores of cobalt and nickel diffused throughout a considerable extent of country.

The region explored comprised a range of rocks composed of alternating strata of talcose and quartzose schists, which crosses the South fork of the Catawba river a little south of the line between Lincoln and Gaston counties, and in the immediate neighborhood of the falls known as the "High Shoals of the Catawba." The general direction of this range, which forms a well defined metalliferous belt of many miles in longitudinal extent, is about N. 20° E. (varying, however, in places between due N. and N. 35° E.), and at the High Shoals it is three or four miles wide. It is everywhere traversed by "veins" of quartz, carrying pyrites and other sulphids, and showing at the surface the limonite *gozzans* derived from their oxydation. These "veins" in some places have all varieties of strike and dip, although the most important ones were found most frequently to *conform in strike*

* The proper date of this paper is August, 1857, at which time it was announced to be read at the Montreal meeting of the Association. Accidents beyond the author's control have delayed its appearance until now.

(or very nearly so) with the containing rocks. The dip of these rocks is nearly vertical, usually a little westerly, although notable contortions and local variations of dip were observed in a few places. At the place where this metalliferous belt crosses the river its boundaries appear to be, on the northwest side a thickly bedded *granitoid* schist, and on the southeast side, forming the barrier over which the water falls at the High Shoals, a massive range of peculiar feldspathic rocks, also thickly bedded in structure, and characterized by having the feldspar crystals, which are numerous and large, all arranged with their longer diameters parallel to the bedded structure, or generally about N. 20° E. On the southwesterly prolongation of this latter rock, wherever it crosses any of the tributaries of the river, a waterfall is found. Crowder's mountain, which towers up about a dozen miles distant in a southwesterly direction, seems to lie on or near this range.

Proceeding to the northeastward from the High Shoals into Lincoln county along this belt of talcose and quartzose schists, many places are encountered where gold has been mined, or washed out from the beds of the small streams, among which may be mentioned the Shuford and Cansler Gold Mines.

Many miles in the distance, but apparently on the same range, is seen the high elevation in which is situated the iron mine known as the "Graham Ore Bank." Fragments of limonite gozzan and honey-combed quartz are constantly encountered on the surface, sometimes isolated, and sometimes strewed along for considerable distances marking outcrops. In this part of the range, the quartz veins are usually found to contain, wherever they have been opened, more or less *galena, blende* and *chalcopyrite*, usually with native gold. In one place *rutile* was found.

Going southwestwardly from the river we find the rocks presenting similar indications, and in the course of some fifteen miles, we encounter successively the "Long Creek Gold Mines," (from one of which, known as the Asbury Shaft, much gold has been taken); and a number of places where iron ore is or has been mined, known as the "Costner Ore Bank;" the "Alison Ore Bank;" the "Ormond Ore Bank;" the "Ferguson Ore Bank," and "Briggs' Ore Bank." A few miles beyond the latter, not far from the same range, lies the well known "Kings Mountain Gold Mine." So called "greenstone-trap dykes" are occasionally encountered, sometimes running parallel to, and sometimes across, the strata. The beds of the streams frequently contain pebbles of *black tourmaline*, and the *black sand*, so common throughout this section of the country, was found to consist here principally of a magnesia tourmaline, easily fusible by the blowpipe. Immense veins, or rather *strata*, of black tourmaline were also observed in several places, usually veined with milky white quartz. This would form a magnificent material for monumental

and ornamental purposes. A turnpike road was found at one spot completely covered and blackened (*paved*, as it were,) for some distance with fragment and blocks of this material, in consequence of its crossing a large outcrop very obliquely. In one place was found the outcrop of a large vein of *ilmenite*. Veins of *pyrites* were found crossing the beds of streams, where the current appeared to have washed them bare, and one such vein was observed which had a well marked *schistose* structure, similar and conformable to that of the imbedding talcose rocks, forming a true *pyrites schist*. In other places solid banks of *limonite* were found, standing in place above the surface of the ground, indicating veins (or strata ?) of pyrites below. Specimens were seen of a crystalline *hematite schist*, said to form a large vein (or stratum ?) in Crowder's mountain, but this was not visited.

It was also perfectly evident, at most of the iron ore beds before mentioned, that the ore was merely the gozzan, or product of oxydation of large strata of *sulphids*, probably *pyrrhotine*, existing below. In some of these places, as at the Ormond, Ferguson and Briggs' Ore Banks, the mining operations have penetrated, in places, to the unaltered, or only partially altered, pyrites. At the Alison and Costner Ore Banks, which are excavations into *strata* of ore from thirty to forty feet in width, the material last thrown out is a true *magnetite schist*, mixed however with much limonite.

Throughout the whole range, wherever examined, the talcose schists were found to contain, in numerous places, small seams, incrustations and stains of a *black substance*, which gave blowpipe reactions for *cobalt*. At every one of the mines above mentioned the ore, or refuse material thrown out, was found to be more or less coated with this substance. At the Ormond Ore Bank, particularly, so much of this substance was found mixed with the ore that it is probably connected with the reputation of the iron produced from this ore, for hardness and toughness, throughout the surrounding country. At the Asbury Shaft of the Long Creek Mines also, masses of quartz thrown out from the vein were found thickly incrusted with mammillary masses of this *wad*, or earthy cobalt. It cannot be doubted that it is the gozzan of some cobaltiferous sulphid existing unaltered in the rocks below. If this substance has ever attracted the attention of mineralogical explorers in this section, it has probably been mistaken for earthy manganese, from which it may readily be distinguished, however, by being very soft, smearing the fingers, and when cut with a knife exhibiting on the section a bright black lustre like that of compact graphite; and to these properties it owes the designation of "black lead," which it bears among the people of the country (to whom it is familiar).

At a spot about a mile in a northeasterly direction from the Long Creek Mines I found, crossing at right angles the road from Lincolnton to Yorkville in S. C., where the latter crosses over an elevation called "Cross'" or the "Paysour mountain," the outcrop of a large "vein," or stratum of the rock, which contains very much of this black gozzan or wad. It can scarcely escape the attention of a person travelling along the road, as it appears like a broad black band at the side of the latter. At this spot it measures about fifteen feet in width. A small opening was made into it three or four rods from the road on the southern side, and it was found to be about twelve feet wide, included between walls of talcose slate, but so highly decomposed that no satisfactory evidence could be obtained of its character where unchanged. It was traced and opened again about half a mile southwesterly from the road, and found to consist there of a number of parallel strata, separated by seams of talcose schist one or two feet wide. The largest of these strata was ten feet wide, presenting a solid bank of limonite, mixed with a little quartz, so compact that it was with difficulty broken by a pick. Following the Yorkville road southerly from the point where this vein crosses it, are encountered, interstratified with the talcose schists, several narrow bands of a granular quartzose schist, corresponding precisely in character to the *itacolumite* of Humboldt, supposed to be the gangue of the *diamond*. No diamonds have however, to my knowledge, yet been found in this immediate neighborhood, although known localities of the gem are not far distant. In places this itacolumite is highly granular and friable, and would be called a *saccharoid* quartz schist. Such specimens call strongly to mind the so-called *flexible sandstone* of Mc Dowell Co., N. C., but no flexible specimens were observed.

Following the vein northwardly from the road, the outcrop was found to descend rapidly along the western slope of Cross' Mountain, and at about a quarter of a mile from the road was found a spot where the ground consisted in great part of fragments of the black cobaltiferous substance. Openings properly made here would probably lead to interesting and valuable developments. A determination of the quantity of mixed oxyds of cobalt and nickel contained in the mineral found at this spot, gave 13·26 per cent. The presence of oxyd of nickel in considerable proportion in this mixture was proved by the method of Liebig,* that is, by passing for some time, in the cold, a strong current of chlorine gas through the solution of the mixed oxyds, to which has been previously added large excesses of cyanid of potassium and caustic soda, the nickel being thus thrown down as a black precipitate of Ni^2O^3. No quantitative determination of the proportion of oxyd of nickel, however, has yet been made.

* Annalen der Chemie und Pharmacie, lxxxvii, 128.

This Cross' Mountain gozzan was found, by qualitative analysis, to contain besides *cobalt*, *nickel*, *manganese* and *iron*, small quantities of *copper*, *bismuth*, *zinc*, *lime*, *alumina*, *magnesia* and *glucina*. Traces of *sulphur* were also ascertained by Mr. Jas. R. Brant, to whom I am indebted for some assistance in the examinations. No traces of arsenic could be found.

In the mineral from the Asbury shaft, qualitative examination detected *iron*, *manganese*, *cobalt*, *nickel*, *copper*, *bismuth*, *zinc*, *alumina*, *silica*, *lime* and *magnesia*, besides traces of something which seemed to give the reactions of *tellurium*.* *Sulphur* in traces was found by Mr. Brant, but arsenic was sought for without success.

The substance from the Ormond Ore Bank may be called a cobaltiferous *earthy manganese*, or granular and amorphous *hausmannite*. It gives with chlorohydric acid a deep brown-black solution with evolution of chlorine (like hausmannite), which so-

* That this is really tellurium seems more probable in view of the unmistakeable indications obtained of the presence of the allied element, bismuth. These two elements, which form the connecting links between the arsenic and sulphur groups, seem, like Ni and Co, Ag and Pb, or Br and I, to preserve (at least in our American localities) a certain degree of concomitance in their occurrence. Thus the first known locality of both Bi and Te in the United States was announced by Prof. B. Silliman, Sen., in the first volume of the Am. Jour. of Science, p. 312, at Huntington, Conn. No further discovery of Te in the United States was recorded until Dr. Jackson's announcement of its existence at Whitehall, near Fredericksburg, Spotsylvania Co., Va., in the Am. J. Science for May, 1848, in the form of *tellurid of bismuth* or tetradymite. Since then tetradymite has been found at other Virginian localities, as the "Tellurium Mine," Fluvanna Co., and the "Monroe Mine," Stafford Co., and by Genth at several places in North Carolina, as at the "Phœnix" and "Boger Mines" in Cabarrus Co., and near the "Washington Mine" in Davidson Co. The only *South American* specimen yet described containing Te was a tellurid of Bi brought to Paris by Claussen from San José in Brazil, in which Damour found 79 p. c. Bi and 16 p. c. Te, with a little Se and S (see Ann. de Ch. et de. Ph., [3], xiii, 372). *Bismuth*, which, it will be observed, was found in small quantity in each of the three specimens examined, appears to be a rather common constituent of our crystalline schists. Besides the "Bismuth Mine" at Huntington, Conn., before alluded to, and the other long known localities at Trumbull and Monroe, Shepard has found it at Haddam, Conn. (Am. J. Sc., [2], xii, 220) and Jackson at the "Lubec Lead Mines" in Maine. The locality at "Brewer's Mine" in Chesterfield District, S. C., the *bismutite* from which was analyzed by Rammelsberg (Pogg. Ann., lxxvi, 564) and Genth (Am. J. Sci., [2], xxiii, 426) has long been known. In Genth's analysis an appreciable quantity of Te was found. Besides the several localities of tetradymite in Virginia and North Carolina before mentioned, Genth found *bismuthine* at Gold Hill, Rowan Co., N. C. (Am. J. Sci., [2], xix, 16), and Jackson states that *bismuth ochre* occurs with the Virginian tetradymites (Dana's Mineralogy, p. 141), and moreover, Genth has very recently (Am. J. Sci., [2], xxiii, 427, May, 1857) brought forward as a new locality of bismutite in North Carolina, *Gaston County* itself, where he says it was discovered by Dr. Asbury of Charlotte, the gentleman, if I mistake not, from whom the "Asbury Shaft" derives its name. In this also Genth obtained indications of the presence of *tellurium*.

With regard to South American localities of bismuth, besides the above-mentioned Brazilian tellurid of Damour, two others have thus far been recorded; one of which is at the mine of San Antonio, near Copiapo, Chili, whence was obtained a *bismuthet of silver*, containing, according to analysis of Domeyko, 10 p. c. of Bi, (Dana, p. 16), and the other is Rammelsberg's *chiviatite*, from Chiviato, Peru, a sulphid of Pb, Cu and Bi, containing 61 p. c. of Bi (Dana, p. 77).

lution contained *iron*, *manganese*, *cobalt*, *nickel*, *copper*, *bismuth*, *alumina*, *glucina*, and traces of *baryta*, *lead* and *magnesia*. There were in this also traces of *sulphur*, but no arsenic.

The Asbury Shaft and Cross' Mountain minerals give deep beautiful grass-green solutions in chlorohydric acid, with evolution of chlorine, which solutions become yellow-brown on adding water, a behavior characteristic of solutions containing considerable quantities of cobalt with iron, and by which these cobaltiferous wads may generally be distinguished from ordinary earthy manganese containing but traces of cobalt or none.

As to the nature of the *unaltered mineral*, from which these cobaltiferous gozzans have been derived by oxydation, it is possible to form a very probable hypothesis. The absence of *arsenic*, not only from these, but from many other minerals that I have examined from this region,* leads to the conclusion that this unaltered mineral must be a *sulphid*, and not an *arseniet* of cobalt and nickel, and the great resemblance of these substances to the cobalt and nickel ore from *Mine la Motte* in Missouri, which is also a product of oxydation, is presumptive evidence that the original mineral may be identical with, or at least similar to, the one existing there. Now this original mineral at Mine la Motte has recently been found by Dr. Genth† to be *siegenite*, containing 30·53 per cent of nickel and 21·34 of cobalt, together with iron and traces of lead, copper and antimony. The fact that the products of oxydation at the Cross' Mountain locality contain but 13 per cent of oxyds of cobalt and nickel is not against this hypothesis, for in the oxydation of such a mixture of sulphids, the produced sulphates of cobalt and nickel, particularly the latter, would be in great measure washed away,‡

* Arsenic indeed appears to be a sparsely distributed constituent of the schists of the Carolinas. Hardly any reliable *analytical* evidences of its occurrence in these latitudes are in existence. We have, however, one such evidence. A mineral found in minute quantities at the bismuth locality in Chesterfield District, S. Carolina, on analysis by Dr. Genth proved to be a sulpharsenate of sulphid of copper, most probably identical with the Peruvian species *enargite* (Am. J. Science, [2], xxiii, 420). Dana also (p. 62) mentions one specimen of *leucopyrite* found in Randolph Co., N. C.

† Am. Jour. Science, [2], xxiii, 419.

‡ It will be remembered that these two sulphates occur naturally, as *bieberite* and *pyromeline*, both products of oxydation of their corresponding sulphids. With regard to the relative tendencies of cobalt and nickel, when in *neutral* or *acid* solution, to undergo oxydation and precipitation as cobaltic and nicklic oxyds, our knowledge is rather meagre, but still experimental evidence bearing upon this point is not wholly wanting. Thus Dr. Gibbs found (Am. J. Science, [2], xiv, 205) that deutoxyd of lead partially oxydizes and precipitates Co from its solutions, but Ni not at all. This suffices to account for the fact that among the numerous analyses of *wads*, *earthy cobalts* and *bog manganese ores* on record, the presence of Ni is seldom or never indicated. The reason why, in *alkaline* solutions, as in the presence of an excess of ammonia or cyanid of potassium, the reverse action takes place, and the Ni is thrown down, while the Co remains in solution, is simply the eminent capacity which cobaltic oxyd possesses to form soluble double salts and conjugated compounds, a tendency apparently not shared by nicklic oxyd.

whilst the iron and manganese, passing to higher states of oxydation, would remain behind in insoluble forms. I cannot therefore refrain from offering the hypothesis that these veins, when opened to a depth below the influence of surface oxydation, will be found to contain either siegenite, or some nearly related species. As relevant to this view, I may cite the fact that Dr. Genth has recently discovered siegenite,* associated with chalcopyrite, pyrites, blende, etc., in the vein worked at the Mineral Hill Mine, Carroll Co., Md., in the same range of metamorphic schists as these North Carolina localities.

The only place in this neighborhood where a vein has been opened to a sufficient depth to expose the character of the unaltered ore, is what is called the "Bronson Shaft," at the Long Creek Mines, which has been recently worked for gold by a New York company, and is situated about half a mile southwesterly from the Asbury Shaft, and either on the same vein with the latter or on a closely parallel one. At this spot, however, unfortunately none of the black cobaltiferous substance occurs with the ore. I examined this mine and found that near the surface and above water level, the mineral mixed with the quartz gangue of the vein is principally limonite, as usual. Below water level, or about forty feet from the surface, the limonite begins to disappear, and the vein gradually assumes the character of a porous mass of quartz filled with strings and bunches of *pyrites* and "vugs" or cavities lined with crystals of the same, sometimes of the octahedral variety. Beautiful specimens were found containing contiguous cavities, some lined with cubic, and others with octahedral crystals, in the same specimen. The whole mass of the vein is here saturated, of course, with water. On descending to the bottom of the shaft, about 140 feet from the surface, the vein is first found *compact* and apparently unaltered, being here about eight feet wide, and composed of a mixture of quartz and *pyrrhotine.*

This pyrrhotine contains, however, but traces of cobalt and nickel. I wish to ask here whether it is not evident from the above facts, that the pyrites, as well as the limonite, found in the upper part of the Bronson shaft, is clearly a product of the action of aerated water upon the pyrrhotine? The pyrrhotine is compact and massive, with a granular fracture,† and no specimens were found in the bottom of the shaft presenting indications of crystallization, whilst all the pyrites found above presents, as before stated, an eminently crystalline structure, an in-

* Am. Jour. of Science, [2], xxiii, 418.

† The fractured surface presents a number of small patches of *chalcopyrite.* I may mention here that Prof. Emmons, in his recent Geological Report on North Carolina (pp. 167, 168) calls this pyrrhotine of the Long Creek Mines "arsenical pyrites." I could find, however, no trace of arsenic. It almost wholly dissolves in warm chlorohydric acid with copious evolution of sulphohydric gas.

dication, according to the crystallogenic views which are rapidly being generally adopted at the present day, of the agency of *water*, at or near the ordinary temperature, in its formation. Considering, for the sake of simplicity, as Breithaupt, von Kobell, Frankenheim and Rammelsberg have done,* that pyrrhotine is *protosulphid* of iron, the action of oxygeniferous water upon it in the formation of pyrites and limonite, may be represented by the rather simple equation

$$3FeS+2\tfrac{1}{2}HO+2O=FeS^2+Fe^2O^3,\ 1\tfrac{1}{2}HO+HS,$$

the sulphohydric acid formed entering into solution in the water, and subsequently either undergoing oxydation in its turn, or issuing in sulphur springs.†

ART. VI.—*On the so-called Triassic Rocks of Kansas and Nebraska;* by F. B. MEEK and F. V. HAYDEN.

IN several of our publications on the geology of Nebraska, we have mentioned a formation (No. 1 of the Nebraska Section) consisting of reddish and yellow sandstones, and various colored clays, with seams and beds of impure lignite, holding a position at the base of the Cretaceous series of the northwest. Although entertaining some doubts respecting the exact age of this formation, we have always placed it provisionally in the Cretaceous system, in our published sections.

Having learned through Mr. Hawn that a precisely similar group of strata, holding apparently the same position, occurs in northeastern Kansas, we placed these latter beds on a parallel with No. 1 of the Nebraska section, in a paper read before the Phila. Acad. Nat. Sci., May, 1857. Soon after the publication of this paper, however, a few fossils Mr. Hawn had shipped to us some time before, from a bed near the base of a section of the Kansas rocks he had furnished us for publication, came to hand. On examining these fossils, we at once discovered they were not, as had been supposed, Cretaceous forms, but similar to those of the Permian of the Old World. From this it became manifest that in drawing a parallel between the Kansas and Nebraska formations, we had carried No. 1 too low in Kansas, by bringing it down so as to include the bed from which these fossils had been obtained.

* Handwörterbuch der Chem. Lieb. Pogg. and Woehler, v, 62.

† These views will be recurred to in a future communication, to be presented whenever I shall have been able to complete my chemical examinations of the minerals collected in this section, and in which I propose to bring forward some general considerations regarding the metamorphism of the crystalline schists of the United States, and the nature and origin of the included metalliferous veins.

This misunderstanding in regard to the lower limits of No. 1, in Kansas, also led us to place on a parallel with that formation, all the lower two-hundred feet of Mr. Marcou's Pyramid Mountain section, (New Mexico) referred by him to the Trias. Suspecting however, that No. 1, as thus defined, might possibly include beds not properly belonging to it, we distinctly stated in the closing remarks of the same paper, that we yet wanted positive evidence, we might not be making it include beds older than any part of the Cretaceous system.

Although we are now aware that in drawing this parallel between the Nebraska rocks and those of Kansas and New Mexico, we carried No. 1, too low, we yet regard all, or nearly all of Mr. Marcou's Pyramid Mountain section, referred by him to the Jurassic System, as equivalent to the Cretaceous formations No. 1, 2 and 3, of Nebraska; while the lower two hundred feet of the Pyramid Mountain referred by Mr. Marcou to the Trias, we think equivalent to the Kansas deposits between the base of No. 1, as we now understand it, and the beds containing the Permian fossils.

In our paper on the collections brought in by Lieut. Warren's expedition to the Black Hills, read before the Acad. Nat. Sci., Philad., March, 1858, we remarked that in consequence of the occurrence in No. 1, of the genus *Baculites*, and numerous leaves closely resembling those of some of the higher types amongst our existing dicotyledonous forest trees, we thought we were hazarding little in referring it to the Cretaceous epoch.

More recently Mr. Hawn has published a paper in the Transactions of the St. Louis Acad. Sci. in which he places this formation in Kansas and New Mexico (as we had done), on a parallel with No. 1, of the Nebraska section, but refers the whole to the Trias.*

This difference of opinion caused us to examine with no little interest, during our recent expedition to Kansas, some of the localities mentioned by Mr. Hawn, near the junction of the Grand Saline and Smoky Hill branches of Kansas river, with the view of determining definitely whether or not the formation regarded by him as Triassic, could really be the same as No. 1 of the Nebraska section. In this we were particularly successful, for we not only found these Kansas formations agreeing exactly in all the details of their lithological characters, with No. 1, in Nebraska, but we also discovered in them several good specimens of the same dicotyledonous leaves so abundant in No. 1, at the mouth of Big Sioux river, and at Blackbird Hill, on the Missouri, in Nebraska. Associated with these leaves we likewise found specimens of the same peculiar trilobate leaf

* Trias of Kansas, by F. Hawn, Trans. St. Louis Acad. Sci., vol. i, p. 171.

(*Ettingshausinia*) mentioned by Mr. Hawn* as occurring in the formation referred by him to the Trias, thus establishing beyond the possibility of a reasonable doubt, the identity of the supposed Triassic deposits of Kansas, and No. 1, of the Nebraska section.

In regard to the leaves here referred to, we would merely remark that they are quite abundant in this formation, both in Nebraska and Kansas, and certainly belong to higher and more modern types of dicotyledonous trees, than have yet been found even in Jurassic rocks. Dr. J. S. Newberry our excellent authority in fossil botany, to whom we have submitted the whole collection, decidedly concurs with us in the opinion that the rocks in which they occur cannot be older than lower Cretaceous. In a communication recently received from him respecting these remains he says: "They include so many highly organized plants, that were there not among them several genera exclusively Cretaceous, I should be disposed to refer them to a more recent era."

"A single glance is sufficient to satisfy any one they are not Triassic. Up to the present time no angiosperm dicotyledonous plants have been found in rocks older than the Cretaceous, while of the eighteen species which comprise your collection sixteen are of this character." * * * *

"The species of your fossil plants are probably all new; though generally closely allied to the Cretaceous species of the Old World. From the limited study I have given them I have referred them to the following genera:

Sphenopteris.	Pyrus?
Abietites.	Alnus.
Acer.	Salix.
Fagus.	Magnolia.
Populus.	Credneria.
Cornus.	Ettingshausinia."
Liriodendron.	

"Of these the last two are exclusively Cretaceous, and highly characteristic of that formation in Europe.

* * * * * * *

"I may say in confirmation of the assertion that your fossil plants are Cretaceous, that I found near the base of the yellow sandstone series in New Mexico, considered Jurassic by Mr. Marcou,—a very similar flora to that represented by your specimens, one species at least being identical with yours, associated with *Gryphœa*, *Inoceramus*, and *Ammonites* of lower Cretaceous species."

* Prof. Swallow exhibited a specimen of this species at the Baltimore meeting of the Am. Association.

We have only to add in regard to the formation under consideration that we think it will no longer be doubted that it really belongs where we have always placed it, in the Cretaceous System.*

Between the base of No. 1 and the beds from which the Permian fossils are obtained in Kansas, there is a considerable thickness of red, blue, green and whitish clays, with a few beds of sandstone, and near the base gypsum deposits. This series may,—at least in part,—be Jurassic or Triassic or both, (much more probably the former), but until we have some reliable palæontological evidence, it would only be groping in the dark to attempt to define its age; knowing as we do that lithological characters are of no value whatever, as a guide in drawing a parallel between these formations and those of the Old World.

As we expect soon to publish a paper giving in more detail the results of our examinations amongst the rocks in which so many Permian fossils have been found in Kansas, we would merely remark here that the coal measures of that region pass upwards by imperceptible gradations into an extensive series of rocks, consisting usually of rather impure more or less magnesian limestones, alternating with generally much thicker beds of blue, green, red and ash-colored laminated clays or very soft shales, with occasional beds of sandstone. Into this series, nearly all the species of fossils found in the middle and intermediate coal measures pass in great numbers.† Associated with these however, we occasionally meet with fossils belonging to types regarded in the Old World as characteristic of the Permian epoch.

* After the reception of a brief preliminary report by us, published last winter in the National Intelligencer, on the collections brought in from the Black Hills by Lieut. Warren, Mr. Marcou published a paper in the *Archives des Sciences de la Bibliotheque Universelle* of Geneva (a translation of which has recently appeared in the New York Mining Journal) in which after speaking of some points of difference in our opinions respecting the geology of the far west, he says, "in other respects the series of Messrs. Meek and Hayden agrees perfectly with mine, and it is with great pleasure I see that these learned geologists admit not only the existence of the New Red Sandstone (Permian and Trias) and Jurassic, but that they are led to regard as Jurassic, formation No. 1, of their Nebraska Cretaceous Series, a formation which from their description, I have no hesitation in regarding as Jurassic."

It was perhaps owing to the necessary brevity of our preliminary statement of the Jurassic and other discoveries in the Black Hills, seen by Mr. Marcou in the Intelligencer, that he misunderstood us. We have no where said we had recognized the Trias in the northwest; nor have we admitted in any of our publications that No. 1, of the Nebraska section is Jurassic. We stated that in consequence of the similarity between the lithological characters of No. 1, and the Jurassic deposits in the Black Hills, and the absence of organic remains near the junction, we were in doubt respecting the particular horizon at which the line should be drawn between them. At the same time, we stated that the beds from which the Jurassic fossils, described by us were obtained, hold a position *below* No. 1, of the Nebraska section.

† Amongst these we recognize nearly all the Carboniferous fossils figured by Mr. Marcou in his "Geology of North America."

As we ascend in this group of strata, which comprises, nearly or quite all the lower Permian, and much of the upper coal measures of Prof. Swallow's and Mr. Hawn's section* we find the Carboniferous forms very gradually diminishing in numbers to be replaced by Permian types, or others rather intermediate in their affinities, between those of the Permian and Carboniferous epochs.

Still higher in the series, without passing any horizon of unconformability, or meeting with any *abrupt* change, either in the fossils, or the lithological characters of the rocks, we find, when fairly up into the Upper Permian of Prof. Swallow's and Mr. Hawn's section, that we have lost sight of nearly, or quite, all the coal measure types, and meet only with Permian forms.

From these facts, we are inclined to the opinion that the entire series, from near the top of the Lower Permian of Prof. Swallow's and Mr. Hawn's section, down even lower than the horizon where they draw the line between the coal measures and the lower Permian,† should be regarded as intermediate in age, and as filling the hiatus between the Permian and upper coal measures of the Old World; while we think only the Upper Permian of their section really represents the Permian rocks, as developed on the other side of the Atlantic.

This intermediate series might be very appropriately termed the Permo-Carboniferous group, to indicate its relations both to the Permian and Carboniferous rocks. In case however, it may be thought best, in order to avoid the inconvenience of introducing a new name into our nomenclature, to class it along with either the Permian or Carboniferous, we would certainly place it in the latter, since Carboniferous types greatly predominated in its fauna.

In conclusion we would state that there is no unconformability so far as our knowledge extends, amongst all the rocks of Nebraska and northeastern Kansas, from the coal measures to the top of the most recent Cretaceous. The whole series in N. E. Kansas, and along the Missouri, as far up as Heart river in Nebraska, where the latest Cretaceous deposits pass beneath the water level, dip to the northwest. Consequently the elevating forces that produced this inclination of these various formations, must have been called into play—as in the region of the Black Hills,—after the close of the Cretaceous epoch, and previous to the deposition of the Miocene Tertiary formations of the northwest.

* Transactions St. Louis Acad. Sci., vol. i, p. 171.

† We found the genus *Monotis* ranging down several hundred feet below the base of what we understand to be the lower Permian in Prof. Swallow's and Mr. Hawn's section.

ART. VII.—*On Lazulite, Pyrophyllite and Tetradymite in Georgia*; by CHARLES UPHAM SHEPARD.

WHILE at Allatoona, Ga., in April last, Mr. S. Harris of that place received a small blue crystal from Dr. Stephenson of Lincoln county, the name of which the Doctor wished to learn. It being handed over to me for determination, I found it to be Lazulite. The form of the crystal was well defined, and wholly different from any specimens I had ever seen from Lincoln county, or from elsewhere. This decided me to visit the locality if possible, on my way back to Charleston. On making the necessary inquiries for my route, I learned with surprise, that instead of coming from Lincoln county, N. C., it was from a county of the same name in Georgia, the two being several hundred miles apart.

Want of time however, prevented my reaching nearer than within twelve miles of the spot; but I was fortunate to obtain the assistance of Dr. Stephenson in procuring a supply of the mineral. He was good enough to visit the locality twice, attended by two miners; and has favored me with a description of the circumstances under which lazulite occurs.

The locality is upon Graves' mountain, a ridge three hundred feet high and two miles in length. This elevation is situated about twelve miles northwest of the short auriferous belt, known as the Columbia gold mines in a county of the same name, lying fifty miles above Augusta. Graves' mountain is supposed to be the southwestern terminus of a second parallel gold belt, which extending across South Carolina, includes the famous Dorr mine, and afterwards runs away into North Carolina. The central part of the mountain, to the thickness of fifty feet, is composed of a hematitic rock, which includes in some places an abundance of a ferruginous kyanite, much resembling in appearance the diaspore from the Urals. With the kyanite is found rutile, often in gigantic crystals (weighing upwards of a pound), and possessed of much regularity of crystalline form. The prevailing figure is a square prism with truncated lateral edges, and surmounted at both extremities by an eight-sided pyramid. There is also found a most remarkably perfect twin crystal, in which the geniculation is six times repeated,—producing an hexagonal prism, surmounted at each end by a six-sided pyramid, with a reëntering, six-sided, hopper-shaped cavity, at the tips. These crystals are all more remarkable for their symmetry and polish, than any I have ever seen. Some are fully equal in lustre to the brilliant crystals of cassiterite from Cornwall or Bohemia. The most perfect rutiles are generally imbedded in the massive kyanite; and when detached leave behind impressions having a

polish and lustre equal to that of their own planes. A little common quartz is also mingled with the kyanite and rutile. Occasionally small imbedded crystals of quartz, of the form of those found in the Trenton limestone of New York, are seen in the kyanite.

Closely associated with kyanite, rutile and quartz, are considerable masses (eight or ten inches thick) of a mineral known among the miners of Georgia as steatite, but which is true pyrophyllite,—differing in no respect from that of the Urals, except in the finer stellulations it presents, and in the slight ferruginous stain it exhibits near their centres.

The hematite is massive, granular (approaching compact); but the masses are somewhat open, from including the decomposing ferruginous kyanite, particles of pyrophyllite and even portions of compact rutile. The large masses consequently possess a somewhat slag-like and roughened aspect; and suggest, on being handled, the presence of some native metal. It is possible that this hematite may contain titanium as a constant ingredient; in which case it may prove a new mineral.

To the southeast of this fifty feet band, appears the itacolumite, with a thickness of more than three hundred feet, which presents numerous included zones or layers, varying from one to three feet in thickness, in which is found imbedded, masses and crystals of lazulite. The continuity of the lazulite is by no means perfect,—the mineral rather exhibiting a tendency to form nests and bunches. Within a few feet of the surface, the rock is loose and sandy, and presents a pale buff color; but at a depth of three feet, it approaches compactness, with a greyish white color. It is obscurely schistose, with a tendency only to cleavage, and at intervals not nearer than two or three inches. The lazulite is almost wholly in crystals, varying from a quarter to one inch in length; and are scattered like garnets through granite or mica slate, presenting a very pleasing appearance from the contrast between the ultramarine blue of the mineral, and the clear, pale buff of the rock.

The itacolumite contains traces of gold, especially near the southern extremity of the formation, where it becomes more schistose and embraces minute crystals of pyrites. It has here been worked to some extent for the precious metal.*

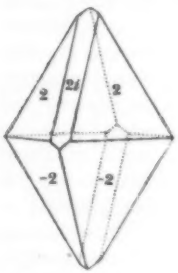
* The aspect of many specimens of the hematitic mixture of kyanite and quartz suggests a resemblance to the diamond gangue of Brazil; and I can but regard this spot as well worthy of being examined for the diamond. Dr. Stephenson informs me that at least ten crystals of this gem have been found in Burke county, two in Habersham, two in Hall and one in Union county. The largest of these is said to have sold for $150 in Philadelphia. The whole number of diamonds thus far found in the United States cannot therefore be less than thirty, nearly all of which occurred in itacolumite.

A greenish, massive kyanite, with scales of white mica (often partially decomposed so as to resemble talc), occurs very rarely in little bunches, in the vicinity of the lazulite crystals. Among these, the naked eye often detects minute and nearly transparent red crystals of rutile. Those of a still smaller size, and visible only with the aid of a lens, are pretty widely diffused through the rock, and often coat the rough surfaces and joints of the lazulite crystals themselves.

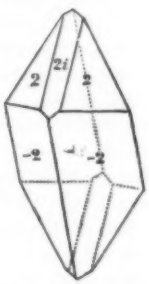
Small drusy cavities very rarely occur in the itacolumite, pretty nearly filled with barytes, massive and crystalline. Very minute and perfectly formed transparent crystals of quartz are discernible in the barytes, likewise microscopic crystals of sulphur. The form of the barytes is that represented in fig. 513 of Dana, coming from the gold formation (itacolumite?) of Fauquier county, Va.

The crystals are represented by the following figures, obligingly furnished by Prof. Dana; and are lettered in accordance with the figure of a lazulite crystal on p. 404 of his Mineralogy. The rarest of these forms is figure 1, of which I have

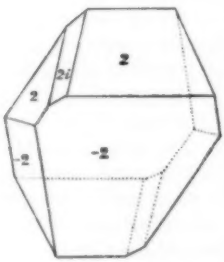
1.



2.



3.



detected only three crystals. Fig. 2 is very common, and is the result of a vertical elongation of the crystal, producing a slightly rhombic prism with very oblique dihedral summits, having slightly truncated edges. The planes $2i$ are always much narrower than in the figures. The crystals of the form of fig. 2 are always small, rarely above one-third of an inch in length. Figs. 3 and 4 are unsymmetrical modifications of fig. 1, in which the right hand planes 2 and –2 are horizontally prolonged, and in fig. 4 are possessed of very unequal dimensions, thus giving rise to a very flattened crystal. The crystals of fig. 3 are the largest found, sometimes measuring above one inch in length.

4.

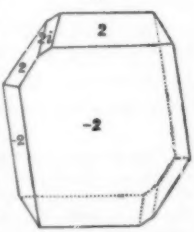
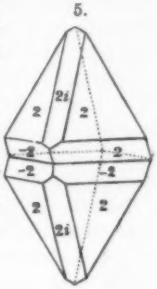


Figure 5, a twin, is by far the most abundant form, equalling in frequency all the others combined. It results from the composition of two such forms as figure 1. The plane of composition is coincident with the rhombic base of the figure; and the angle of revolution =180°.

5.



The faces of none of these crystals are sufficiently polished to allow of the use of the reflecting gonimeter.

The color of the lazulite is various shades of berlin and indigo blue. The effect of weathering is, to lower the intensity of the blue, and rarely to give rise to a shade of green.

Tetradymite in Lumpkin county.

An important discovery of gold has been made during the last summer in the middle of the Chestatee river, four miles east of Dahlonega. It occurs in seams in hornblendic gneiss. Accompanying the gold were found considerable quantities of a white foliated massive mineral, having nearly the color and lustre of tin, which was taken for silver by the miners, though to others it suggested the idea of platinum and even of molybdenite. Several specimens were forwarded to me for examination by Dr. M. F. Stephenson and by W. F. Harris, Esq. I find it to be tetradymite, a species I had before observed in small quantities along with gold, at the Pascoe mine in Cherokee county, and at a place near Van Wort in Polk county.

From the specimens sent, it appears that the gangue of the mineral is gneiss; though in a specimen from Mr. Harris it is diffused in seams through granular white calcite, rendering the mass as heavy as barytes, for which substance it appears, on this account, to have been mistaken.

In both gangues, it is attended by gold.* A number of specimens of the gneiss included with quartz veins, were sent to me for inspection. The quartz veins are transverse to the stratification of the gneiss, and vary in thickness from one to two inches. They contain irregularly shaped masses of pyrrhotine (intermingled with traces of chalcopyrite), chlorite, angular fragments of green hornblendic gneiss, cleavable calcite, ilmenite in broad highly curved crystals, to which may be added a few crystals of allanite and grains of yellow apatite.† A few

* As it has a very pale brass-yellow color and assays at the mint only from 717 to 800, while the deposit gold in the vicinity varies from 910 to 920, it is possible it may be an alloy of gold and bismuth, analogous to the sample I examined several years ago from the Charlotte, N. C., mint, and for which, in the event of its occuring in nature, I have suggested the name of Bismuthaurite.

† This is the only instance in which I have detected apatite in the Southern states; where beryl, also, appears to be equally rare.

grains of reddish garnet are also visible in the gneiss, near its junction with the quartz veins.

The tetradymite is a very handsome metallic mineral. It is broadly laminated for the most part, though sometimes approaching granular in structure. In one instance it is reported to have occurred in foliæ three or four inches across. I have not seen it in perfect crystals. It contains very rarely, minute forms of a silver-white pyrites not yet determined. The tetradymite when first heated before the blowpipe, evolves a distinct odor of selenium.

When in granular calcite, it is accompanied by small crystals of a brownish, semi-transparent mineral, in prisms resembling tourmaline, but whether they belong to this species I have been unable to determine.

Leadhillite occurs in small quantity at the Morgan silver lead mine, in Spartanburg district, South Carolina. It is associated with pyromorphite and cerusite.

Art. VIII.—*Address by Lord Brougham on the Inauguration of a Statue to Sir Isaac Newton.**

To record the names and preserve the memory of those whose great achievements in science, in arts, or in arms have conferred benefits and lustre upon our kind, has in all ages been regarded as a duty and felt as a gratification by wise and reflecting men. The desire of inspiring an ambition to emulate such examples generally mingles itself with these sentiments; but they cease

* From the London Times, September 23.

Grantham, Tuesday, September 21, 1858.—Lincolnshire enjoys the proud distinction of having given to the world the illustrious mathematician and philosopher, Sir Isaac Newton—justly described as "the greatest genius of the human race"—who was born at the manor-house of Woolsthorpe, a hamlet eight miles from this town, on Christmas Day, 1642. Sir Isaac was a posthumous child, his father having died, at a comparatively early age, some three months before the birth of a son whose reputation will endure "to the last syllable of recorded time." Mrs. Newton re-married, and the embryo philosopher seems to have remained under the care of his maternal grandmother and uncle until he attained the age of twelve, when he was sent to the grammar-school at Grantham. While at school he displayed an extraordinary inclination for mechanics, and busied himself, during the time devoted by his schoolmates to play, in making models of various kinds, chiefly clocks and sun dials, one of the latter of which is still to be seen carved upon the walls of the old manor-house at Woolsthorpe. He was entered, in 1661, at Trinity College, Cambridge, where he was fortunate enough to secure the friendship of the learned Dr. Isaac Barrow, who had been elected Greek Professor in 1660, and who became Lucasian Professor in 1663. In the autumn of 1667, Newton was elected a minor fellow; on the 16th of March, 1668, he was elected a major fellow; and on the 29th of October, 1669, he was appointed Lucasian Professor, in the room of Dr. Barrow, who is said to have resigned with a view to his appointment, and from this period may be dated the development of those wonderful scientific discoveries which have given him a world-wide and time-enduring reputation. It is unnecessary to trace further the career of this great philosopher, over whose giant intellect a sad

not to operate even in the rare instances of transcendent merit, where matchless genius excludes all possibility of imitation, and nothing remains but wonder in those who contemplate its triumphs at a distance that forbids all attempts to approach. We are this day assembled to commemorate him of whom the consent of nations has declared that he is chargeable with nothing like a follower's exaggeration or local partiality, who pronounces the name of Newton as that of the greatest genius ever bestowed by the bounty of Providence for instructing mankind on the frame of the universe, and the laws by which it is governed:

> "Qui genus humanum ingenio superavit, et omnes
> Restinxit; stellas exortus uti ætherius sol."—(*Luc.*)
>
> "In genius who surpassed mankind as far
> As does the mid-day sun the midnight star."—(*Dryden.*)

But, though scaling these lofty heights be hopeless, yet is there

cloud subsequently passed, but who died at a green old age, in his 85th year, but unmarried, on the 20th of March, 1727.

The relations of Sir Isaac, who inherited his personal estate, devoted the sum of £500 to the erection of a monument to his memory in Westminister Abbey, but in his case the proverb that a prophet is honored everywhere save in his own country and among his own people, has, until recently, been verified. Some three or four years ago, however, the inhabitants, or the Town Council, of Grantham, bethought themselves that some ornament was required for a vacant space of ground which is styled St. Peter's Hill, though it seems to be little, if at all, above the dead level of the Lincolnshire fens. It was suggested, and the suggestion was favorably received, that the most appropriate ornament would be a monument to the memory of a man whose early career was so closely identified with the town and neighborhood, and whose researches had conferred an eternal benefit upon mankind. A committee was formed to carry out this object, and Mr. Thomas Winter, a member of the Town Council—to whose untiring zeal and energy its successful accomplishment is, we believe, mainly attributable—undertook to act as the honorary secretary. Mr. Winter at once placed himself in communication with Lord Rosse, Lord Brougham and other gentlemen of distinction in the literary and scientific world, who evinced a warm interest in the success of the scheme. Under these auspices the project received the sanction of the Royal Society, and the patronage of Her Majesty and the Prince Consort, who aided the fund by a subscription of £100. A general meeting of the subscribers was held in 1854, at St. George's Hall, Liverpool, during the *séance* in that town of the British Association for the Advancement of Science, when it was resolved that the memorial should be a bronze statue, and its execution was intrusted by the Committee of Selection to Mr. William Theed, the result of whose labors is not only creditable to himself, but not unworthy of the great philosopher whose memory it perpetuates. A detailed description of the statue appeared in the *Times* of Thursday last. The likeness of Sir Isaac is copied from the mask of his face taken after death, and from a portrait bust by Roubilliac. It represents him in the costume of the period, and in the gown of a Master of Arts, in the act of lecturing. The figure is nearly thirteen feet high, weighing upwards of two tons, and about half the quantity of the material of which it is composed was presented in the shape of old gun metal, by Her Majesty's government. The statue was cast at the foundery of Messrs. Robinson and Cottam, of Pimlico, and as a specimen of clean casting, with sharp outline, does them high credit. The figure stands upon a pedestal of Anglesey marble, designed by Mr. Theed, and cut by Mr. Rogers, of Park Hill. The total height of the pedestal and figure is twenty-seven feet, and its cost is £1,630, of which £600 were contributed by the inhabitants of Grantham and the neighborhood.

some use and much gratification in contemplating by what steps he ascended. Tracing his course of action may help others to gain the lower eminences lying within their reach, while admiration excited and curiosity satisfied are frames of mind both wholesome and pleasing. Nothing new, it is true, can be given in narrative, hardly anything in reflection, less still perhaps in comment or illustration; but it is well to assemble in one view various parts of the vast subject, with the surrounding circumstances, whether accidental or intrinsic, and to mark in passing the misconceptions raised by individual ignorance or national prejudice, which the historian of science occasionally finds crossing his path. The remark is common and is obvious, that the genius of Newton did not manifest itself at a very early age. His faculties were not, like those of some great and many ordinary individuals, precociously developed. Among the former, Clairaut stands preëminent, who at nineteen years of age presented to the Royal Academy a memoir of great originality upon a difficult subject in the higher geometry, and at eighteen published his great work on curves of double curvature, composed during the two preceding years. Pascal, too, at sixteen, wrote an excellent treatise on conic sections. That Newton cannot be ranked in this respect with those extraordinary persons is owing to the accidents which prevented him from entering upon mathematical study before his eighteenth year; and then a much greater marvel was wrought than even the Clairauts and the Pascals displayed.

His earliest history is involved in some obscurity, and the most celebrated of men has, in this particular, been compared to the most celebrated of rivers (the Nile), as if the course of both in its feebler state had been concealed from mortal eyes. We have it, however, well ascertained that within four years, between the ages of eighteen and twenty-two, he had begun to study mathematical science, and had taken his place among its greatest masters; learnt for the first time the elements of geometry and analysis, and discovered a calculus which entirely changed the face of the science, effecting a complete revolution in that and in every branch of philosophy connected with it. Before 1661 he had not read *Euclid;* in 1665 he had committed to writing the method of fluxions. At twenty-five years of age he had discovered the law of gravitation, and laid the foundation of celestial dynamics, the science created by him. Before ten years had elapsed he added to his discoveries that of the fundamental properties of light. So brilliant a course of discovery in so short a time, changing and reconstructing analytical, astronomical, and optical science, almost defies belief. The statement could only be deemed possible by an appeal to the incontestible evidence that proves it strictly true. By a rare felicity these

doctrines gained the universal assent of mankind as soon as they were clearly understood; and their originality has never been seriously called in question.

Some doubts having been raised respecting his inventing the calculus—doubts raised in consequence of his so long withholding the publication of his method—no sooner was the inquiry instituted than the evidence produced proved so decisive that all men in all countries acknowledged him to have been by several years the earliest inventor, and Leibnitz at the utmost, the first publisher, the only question raised being, first, whether or not he had borrowed from Newton; and next, whether, as second inventor he could have any merit at all,—both which questions have long since been decided in favor of Leibnitz. But undeniable though it be that Newton made the great steps of this progress, and made them without any anticipation or participation by others, it is equally certain that there had been approaches in former times by preceding philosophers to the same discoveries. Cavalieri, by his Geometry of Indivisibles (1635), Roberval, by his Method of Tangents (1367), had both given solutions which Descartes could not attempt; and it is remarkable that Cavalieri regarded curves as polygons, surfaces as composed of lines, while Roberval viewed geometrical quantities as generated by motion; so that the one approached to the differential calculus, the other to fluxions; and Fermat, in the interval between them, comes still nearer the great discovery by his determination of *maxima* and *minima*, and his drawing of tangents. More recently Hudden had made public similar methods invented by Schœtin; and what is material, treating the subject algebraically, while those just now mentioned had rather dealt with it geometrically. It is thus easy to perceive how near an approach had been made to the calculus before the great event of its final discovery. There had in like manner been approaches made to the law of gravitation, and the dynamical system of the universe. Galileo's important propositions on motion, especially on the curvilinear motion, and Kepler's laws upon the elliptical form of the planetary orbits, the proportion of the areas to the times, and of the periodic times to the mean distances; and Huygens's theorems on centrifugal forces had been followed by still nearer approaches to the doctrine of attraction. Borelli had distinctly ascribed the motion of the satellites to their being drawn towards the principal planets, and thus prevented from flying off by the centrifugal force. Even the composition of white light, and the different action of bodies upon its component parts had been vaguely conjectured by Ant. de Dominis, Archbishop of Spalatro, at the beginning, and more precisely in the middle of the 17th century by Marcus (Kronland, of Prague,) unknown to Newton, who only refers to the

Archbishop's work; while the treatise of Huygens on light, Grimaldi's observation on colors by inflexion, as well as on the elongation of the image in the prismatic spectrum, had been brought to his attention, although much less near to his own great discovery than Marcus's experiment.

But all this only shows that the discoveries of Newton, great and rapid as were the steps by which they advanced our knowledge, yet obeyed the law of continuity, or rather of gradual progress, which governs all human approaches towards perfection. The limited nature of man's faculties precludes the possibility of his ever reaching at once the utmost excellence of which they are capable. Survey the whole circle of the sciences, and trace the history of our progress in each, you find this to be the universal rule. In chemical philosophy the dreams of the Alchemists prepared the way for the more rational, though erroneous, theory of Stahl; and it was by repeated improvements that his errors, so long prevalent, were at length exploded, giving place to the sound doctrine which is now established. The great discoveries of Black and Priestley, on heat and aeriform fluids had been preceded by the happy conjectures of Newton and the experiments of others. Nay, Voltaire had well nigh discovered both the absorption of heat, the constitution of the atmosphere, and the oxydation of metals; and by a few more trials might have ascertained it. Cuvier had been preceded by inquirers who took sound views of fossil osteology, among whom the truly original genius of Hunter fills the foremost place. The inductive system of Bacon had been, at least in its practice, known to his predecessors. Observations and even experiments were not unknown to the ancient philosophers, though mingled with gross errors; in early times, almost in the dark ages, experimental inquiries had been carried on with success by Friar Bacon, and that method actually recommended in a treatise, as it was two centuries later by Leonardo da Vinci, and at the latter end of the next century Gilbert examined the whole subject of magnetic action entirely by experiments. So that Lord Bacon's claim to be regarded as the father of modern philosophy rests upon the important, the invaluable step of reducing to a system the method of investigation adopted by those eminent men, generalizing it, and extending its application to all matters of contingent truth, exploding the errors, the absurd dogmas, and fantastic subtleties of the ancient schools, and above all, confining to the subject of our inquiry, and the manner of conducting it, within the limits which our faculties prescribe. Nor is this great law of gradual progress confined to the physical sciences; in the moral it equally governs. Before the foundations of political economy were laid by Hume and Smith, a great step had been made by the French philosophers, disciples of

Quesnai; but a nearer approach to sound principles had signalized the labors of Gournay, and those labors had been shared and his doctrines patronized by Turgôt when Chief Minister. Again, in constitutional policy, see by what slow degrees, from its first rude elements, the attendance of feudal tenants at their lord's court, and the summons of burghers to grant supplies of money, the great discovery of modern times in the science of practical politics has been effected, the representative scheme which enables states of any extent to enjoy popular government and allows mixed monarchy to be established, combining freedom with order—a plan pronounced by the statesmen and writers of antiquity to be of hardly possible formation, and wholly impossible continuance. The globe itself, as well as the science of its inhabitants, has been explored according to the law which forbids a sudden and rapid leaping forward, and decrees that each successive step, prepared by the last, shall facilitate the next. Even Columbus followed several successful discoverers on a smaller scale, and is by some believed to have had, unknown to him, a predecessor in the great exploit by which he pierced the night of ages, and unfolded a new world to the eyes of the old. The arts afford no exception to the general law. Demosthenes had eminent forerunners, Pericles the last of them. Homer must have had predecessors of great merit, though doubtless as far surpassed by him as Fra Bartolomeo and Pietro Perugino were by Michael Angelo and Raphael. Dante owed much to Virgil; he may be allowed to have owed, through his Latin Mentor, not a little to the old Grecian; and Milton had both the orators and the poets of the ancient world for his predecessors and his masters. The art of war itself is no exception to the rule. The plan of bringing an overpowering force to bear on a given point had been tried occasionally before Frederick II. reduced it to a system; and the Wellingtons and Napoleons of our own day made it the foundation of their strategy as it had also been previously the main-spring of our naval tactics.

It has oftentimes been held that the invention of logarithms stands alone in the history of science, as having been preceded by no step leading towards the discovery. There is, however, great inaccuracy in this statement, for not only was the doctrine of infinitesimals familiar to its illustrious author, and the relation of geometrical to arithmetical series well known, but he had himself struck out several methods of great ingenuity and utility (as that known by the name of Napier's Bones)—methods that are now forgotten, eclipsed as they were by the consummation which has immortalized his name. So the inventive powers of Watt, preceded as he was by Worcester and Newcomen, but far more materially by Causs and Papin, had been exercised on

some admirable contrivances, now forgotten, before he made the step which created the steam-engine anew—not only the parallel motion, possibly a corollary to the proposition on circular motion in the *Principia*, but the separate condensation, and above all, the governor, perhaps the most exquisite of mechanical inventions; and now we have those here present who apply the like principle to the diffusion of knowledge, aware, as they must be, that its expansion has the same happy effect, naturally preventing mischief from its excess which the skill of the great mechanist gave artificially to steam, thus rendering his engine as safe as it is powerful.

The grand difference, then, between one discovery or invention and another is in degree rather than in kind; the degree in which a person, while he outstrips those whom he comes after, also lives, as it were, before his age. Nor can any doubt exist that, in this respect, Newton stands at the head of all who have extended the bounds of knowledge. The sciences of dynamics and of optics are especially to be regarded in this point of view, but the former in particular; and the completeness of the system which he unfolded, its having been at the first elaborated and given in perfection, its having, however new, stood the test of time, and survived, nay gained by, the most rigorous scrutiny, can be predicated of this system alone, at least in its high degree. That the calculus, and those parts of dynamics which are purely mathematical, should thus endure for ever is a matter of course. But his system of the universe rests partly upon contingent truths, and might have yielded to new experiments and more extended observation. Nay, at times it has been thought to fail, and further investigation was deemed requisite to ascertain if any error had been introduced—if any circumstance had escaped the notice of the great founder. The most memorable instance of this kind is the discrepancy supposed to have been found between the theory and the fact in the motion of the lunar apsides, which about the middle of the last century occupied the three first analysts of the age. The error was discovered by themselves to have been their own in the process of their investigation; and this, like all the other doubts that were ever momentarily entertained, only led in each instance to new and more brilliant triumphs of the system. The prodigious superiority in this cardinal point of the Newtonian to other discoveries, appears manifest upon examining almost any of the chapters in the history of science. Successive improvements have, by extending our views, constantly displaced the system that appeared firmly established. To take a familiar instance, how little remains of Lavoisier's doctrine of combustion and acidification, except the negative positions, the subversion of the system of Stahl! The substance having most eminently the prop-

erties of an acid (chlorine) is found to have no oxygen at all, while many substances abounding in oxygen, including alkalies themselves, have no acid property whatever; and without the access of oxygenous or of any other gas heat and flame are produced in excess.

The doctrines of free trade had not long been promulgated by Smith before Bentham demonstrated that his exception of usury was groundless; and his theory has been repeatedly proved erroneous on colonial establishments, as well as his exception to it on the navigation laws; and the imperfection of his views on the nature of rent is undeniable, as well as on the principle of population. In these and such instances as these it would not be easy to find in the original doctrines the means of correcting subsequent errors, or the germs of extended discovery. But even if philosophers finally adopt the undulatory theory of light instead of the atomic, it must be borne in mind that Newton gave the first elements of it by the well known proposition in the 8th section of the Second Book of the *Principia*, the scholium to that section also indicating his expectation that it would be applied to optical science; while Biot has shown how the doctrine of fits of reflection and transmission tallies with polarization, if not with undulation also.

But the most marvellous attribute of Newton's discoveries, that in which they stand out prominent among all the other feats of scientific research, stamped with the peculiarity of his intellectual character—is this, that their great author lived before his age, anticipating in part what was long after wholly accomplished, and thus unfolding some things which at the time could be but imperfectly, others, not at all, comprehended, and not rarely pointing out the path and affording the means of treading it to the ascertainment of truths then veiled in darkness. He not only enlarged the actual dominion of knowledge, penetrating to regions never before explored, and taking with a firm hand undisputed possession; but he showed how the bounds of the visible horizon might be yet further extended, and enabled his successors to occupy what he could only descry; as the illustrious discoverer of the new world made the inhabitants of the old cast their eyes over lands and seas far distant from those he had traversed; lands and seas of which they could form to themselves no conception, any more than they had been able to comprehend the course by which he led them on his grand enterprise. In this achievement, and in the qualities which alone made it possible, inexhaustible fertility of resources, patience unsubdued, close meditation that would suffer no distraction, steady determination to pursue paths that seemed all but hopeless, and unflinching courage to declare the truths they led to, how far soever removed from ordinary apprehension—in

these characteristics of high and original genius we may be permitted to compare the career of those great men. But Columbus did not invent the mariner's compass as Newton did the instrument which guided his course and enabled him to make his discoveries, and his successors to extend them by closely following his directions in using it. Nor did the compass suffice to the great navigator without making any observations, though he dared to steer without a chart; while it is certain that by the philosopher's instrument his discoveries were extended over the whole system of the universe, determining the masses, the forms, and the motions of all its parts by the mere inspection of abstract calculations and formulas analytically deduced. The two great improvements in this instrument which have been made—the calculus of variations by Euler and Lagrange, the method of partial differences by d'Alembert—we have every reason to believe were known at least in part to Newton himself. His having solved an isoperimetrical problem (finding the line whose revolution forms the solid of least resistance,) shows clearly that he must have made the coordinates of the generating curve vary, and his construction agrees exactly with the equation given by that calculus. That he must have tried the process of integrating by parts in attempting to generalize the inverse problem of central forces before he had recourse to the geometrical approximation which he has given, and also when he sought the means of ascertaining the comet's path, which he has termed by far the most difficult of problems, is eminently probable, when we consider how naturally that method flows from the ordinary process for differentiating compound quantities, by supposing each variable in succession constant; in short, differentiating by parts. As to the calculus of variations having substantially been known to him no doubt can be entertained. Again: in estimating the ellipticity of the earth, he proceeded upon the assumption of a proposition, of which he gave no demonstration, (any more than he had done of the isoperimetrical problem,) that the ratio of the centrifugal force to gravitation determines the ellipticity.

Half a century later, that which no one before knew to be true, which many probably considered to be erroneous, was examined by one of his most distinguished followers, Maclaurin, and demonstrated most satisfactorily to be true. Newton had not failed to perceive the necessary effects of gravitation in producing other phenomena beside the regular motion of the planets and their satellites in their course round their several centres of attraction. One of these phenomena, wholly unsuspected before the discovery of the general law, is the alternate movement to and fro of the earth's axis, in consequence of the solar (and also of the lunar) attraction combined with the earth's motion. This

libration, or nutation, distinctly announced by him as the result of the theory, was not found by actual observation to exist till sixty years and upwards had elapsed, when Bradley proved the fact. The great discoveries which have been made by Lagrange and Laplace upon the results of disturbing forces have established the law of periodical variation of orbits, which secures the stability of the system by prescribing a *maximum* and a *minimum* amount of deviation; and this is not a contingent, but a necessary truth, by rigorous demonstration, the inevitable result of undoubted *data* in point of fact, the eccentricities of the orbits, the directions of the motions, and the movement in one plane of a certain position. That wonderful proposition of Newton, which, with its corollaries, may be said to give the whole doctrine of disturbing forces, has been little more than applied and extended by the labors of succeeding geometricians. Indeed, La Place, struck with wonder at one of his comprehensive general statements on disturbing forces in another proposition, has not hesitated to assert that it contains the germ of Lagrange's celebrated inquiry exactly a century after the *Principia* was given to the world.

The wonderful powers of generalization, combined with the boldness of never shrinking from a conclusion that seemed the legitimate result of his investigations—how new and even startling soever it might appear—was strikingly shown in that memorable inference which he drew from optical phenomena, that the diamond is "an unctuous substance coagulated"; subsequent discoveries having proved both that such substances are carbonaceous, and that the diamond is crystallized carbon; and the foundations of mechanical chemistry were laid by him with the boldest induction and most felicitous anticipations of what has since been effected. The solution of the inverse problem of disturbing forces has led Le Verrier and Adams to the discovery of a new planet, merely by deductions from the manner in which the motions of an old one are affected, and its orbit has been so calculated that observers could find it—nay, its disc as measured by them varies less than a second from the amount given by the theory. Moreover, when Newton gave his estimate of the earth's density, he wrote a century before Maskelyne, who, by measuring the force of gravitation in the Scotch mountains, gave the proportion to water as 4·716 to 1; and, many years after, Cavendish, by experiments with mechanical apparatus, (1798) corrected this to 5·48, and Baily, more recently (1842) to 5·66, Newton having given the proportion as between five and six times. In these instances he only showed the way and anticipated the result of future inquiry by his followers. But the oblate figure of the earth affords an example of the same kind, with this difference, that here he has himself perfected the discovery and nearly completed the demonstration. From the

mutual gravitation of the particles which form its mass, combined with their motion round its axis, he deduced the proposition that it must be flattened at the poles; and he calculated the proportion of its polar to its equatorial diameter. By a most refined process he gave this proportion upon the supposition of the mass being homogeneous. That the proportion is different in consequence of the mass being heterogeneous does not in the least affect the soundness of his conclusion. Accurate measurements of a degree of latitude in the equatorial and polar regions, with experiments on the force of gravitation in those regions, by the different lengths of a pendulum vibrating seconds, have shown that the excess of the equatorial diameter is about 11 miles less than he had deduced it from the theory; and thus that the globe is not homogeneous. But on the assumption of a fluid mass, the ground of his hydrostatical investigation, his proportion of 229 to 230 remains unshaken, and is precisely the one adopted and reasoned from by Laplace, after all the improvements and all the discoveries of later times. Surely at this we may well stand amazed, if not awe-struck.

A century of study, of improvement, of discovery, has passed away, and we find Laplace master of all the new resources of the calculus, and occupying the heights to which the labors of Euler, Clairaut, D'Alembert and Lagrange have enabled us to ascend, adopting the Newtonian fraction of 1: 230 as the accurate solution of this speculative problem. New admeasurements have been undertaken upon a vast scale, patronised by the munificence of rival governments; new experiments have been performed with approved apparatus of exquisite delicacy; new observations have been accumulated, with glasses far exceeding any powers possessed by the resources of optics in the days of him to whom the science of optics as well as dynamics owes its origin; the theory and the fact have thus been compared and reconciled together in more perfect harmony; but that theory has remained unimproved, and the great principle of gravitation, with its most sublime results, now stands in the attitude, and of the dimensions, and with the symmetry, which both the law and its application received at once from the mighty hand of its immortal author. But the contemplation of Newton's discoveries raises other feelings than wonder at his matchless genius. The light with which it shines is not more dazzling than useful. The difficulties of his course and his expedients, alike copious and refined for surmounting them, exercise the faculties of the wise while commanding their admiration. But the results of his investigations, often abstruse, are truths so grand and comprehensive, yet so plain, that they both captivate and instruct the simple. The gratitude, too, which they inspire, and the veneration with which they encircle his name, far from tending to obstruct future improvement, only proclaim his disciples the

zealous because rational followers of one whose example both encouraged and enabled his successors to make further progress. How unlike the blind devotion to a master which for so many ages of the modern world paralysed the energies of the human mind!—

> "Had we still paid that homage to a name
> Which only God and Nature justly claim,
> The western seas had been our utmost bound,
> The poets still might dream the sun was drown'd,
> And all the stars that shine in southern skies
> Had been admired by none but savage eyes."

Nor let it be imagined that the feelings of wonder excited by contemplating the achievements of this great man are in any degree whatever the result of national partiality, and confined to the country which glories in having given him birth. The language which expresses her veneration is equalled, perhaps exceeded, by that in which other nations give utterance to theirs, not merely by the general voice, but by the well-considered and well-informed judgment of the masters of science. Leibnitz, when asked at the royal table in Berlin his opinion of Newton, said that, "taking mathematicians from the beginning of the world to the time when Newton lived, what he had done was much the better half." "The *Principia* will ever remain a monument of the profound genius which revealed to us the greatest law of the universe," are the words of Laplace. "That work stands preëminent above all the other productions of the human mind." "The discovery of that simple and general law, by the greatness and the variety of the objects which it embraces, confers honor upon the intellect of man." Lagrange, we are told by D'Alembert, was wont to describe Newton as the greatest genius that ever existed, but to add "how fortunate he was also, because there can only once be found a system of the universe to establish." "Never," says the father of the Institute of France—one filling a high place among the most eminent of its members—"Never," says M. Biot, "was the supremacy of intellect so justly established and so fully confessed. In mathematical and in experimental science without an equal and without an example, combining the genius for both in its highest degree." The *Principia* he terms the greatest work ever produced by the mind of man, adding, in the words of Halley, "that a nearer approach to the Divine nature has not been permitted to mortals." "In first giving to the world Newton's method of fluxions," says Fontenelle, "Leibnitz did like Prometheus—he stole fire from Heaven to bestow it upon men." "Does Newton," L'Hôpital asked, "sleep and wake like other men? I figure him to myself as a celestial genius, entirely disengaged from matter."

To so renowned a benefactor of the world, thus exalted to the loftiest place by the common consent of all men—one whose life,

without the intermission of an hour, was passed in the search after truths the most important, and at whose hands the human race had received only good, never evil—it is befitting that no memorial should have been raised by nations which erect statues to the tyrants and conquerors, the scourges of mankind. * * * * But that his own countrymen justly proud of having lived in his time, should have left this duty to their successors, after a century and a half of professed veneration and lip homage, may well be deemed strange. The inscription upon the cathedral, masterpiece of his celebrated friend's architecture, may possibly be applied in defence of this neglect: "If you seek for a monument, look around." "If you seek for a monument, lift up your eyes to the heavens, which show forth his fame." Nor, when we recollect the Greek orator's exclamation, "The whole earth is the monument of illustrious men," can we stop short of declaring that the whole universe is Newton's. Yet in raising the statue which preserves his likeness, near the place of his birth, on the spot where his prodigious faculties were unfolded and trained, we at once gratify our honest pride as citizens of the same state, and humbly testify our grateful sense of the Divine goodness which deigned to bestow upon our race one so marvellously gifted to comprehend the works of Infinite Wisdom, and so piously resolved to make all his study of them the source of religious contemplations, both philosophical and sublime.

ART. IX.—*Description of a new Mineral Species from Chili;* by FREDERICK FIELD. (From a letter to J. D. DANA, dated Guyacana, Coquimbo, Chili, September 6, 1858).

I SEND you a specimen of a mineral from the Cordilleras of Chili, which appears to me highly interesting. It consists entirely of copper, arsenic and sulphur, having the following composition:

Copper,	48·50
Sulphur,	31·82
Arsenic,	19·14
Iron, silver,	*traces.*
	99·46

and consequently has the following formula: $3Cu_2S+AsS_5$, and may be considered as a tribasic sulpharseniate of copper, like the artificial tribasic sulpharseniate of potassium, in which that metal is replaced by Cu_2. Hardness 3·5—4. Sp. gr. 4·39.

You will see it resembles Tennantite in which the arsenic takes the place of the iron; a specimen of Tennantite having the following value: Cu 48·2, As 12·5, Fe 9·0, S 31·14. I have proposed the name "*Guayacanite*" for this new species, as the mineral was first brought to the large copper smelting works of Guayacana.

Art. X.—*Geographical Notices.* No. V.

Recent Surveys of the Amoor River.—The opening of the Chinese Empire, the negotiation of a commercial treaty with Japan, and the spread of the Russian dominion over the Amoor region and Manchooria, in respect to which intelligence has recently been received, are events which combine to give peculiar interest and importance to our meagre knowledge of Eastern Asia.

The proceedings in China and Japan have attracted universal attention. The advances of Russia, however, in developing the resources of its legitimate territory and in acquiring new domains, have been conducted in a manner so quiet as to escape general attention and elude the opposition of diplomatic vigilance. Siberia has been so little known, and so much depreciated by the world at large, that the accession of some thousands of square miles to its territory has passed almost unnoticed. But it will not be many years before the Russian policy on the Amoor river will be appreciated as it deserves, and already the demand has become urgent in this country for definite knowledge in respect to a region with which American relations are likely to grow continually more intimate.

To satisfy in part such inquiries, the Government at Washington has lately published (Wash. 67 pp., 8vo) a "Report of Explorations on the Amoor river," which were made last year by Mr. P. McD. Collins, an American citizen, who received from the President, in 1856, an appointment, without a post, as "United States Commercial Agent for the Amoor river," and who travelled over land from the Baltic to the Pacific, endeavoring to ascertain what relations might be established with advantage between our own country and the possessions of Russia in Eastern Asia.

The official rank of Mr. Collins gave him opportunities of intercourse with Gen. Mouravieff, the Governor of Eastern Siberia, and with many other dignitaries, from whom he gathered some important facts in respect to the Russian policy in that region. In addition to these, he states his own observations made on a hurried tour through a region of vast extent and varied resources. His report accordingly will be valuable to those who are interested in political changes, and to American merchants, but the circumstances under which it was prepared were by no means favorable to the collection of scientific materials. Three hastily constructed maps, (without the indications of latitude and longitude,) appended to the volume, add almost nothing to our knowledge.

Mr. Collins crossed the Urals at Ecatherinberg, and then proceeded by Tumen, Omsk and Tomsk to Irkootsk, where he remained a month. He then visited Kyatcha and Mai-mat-tschin, neighboring towns,—the former Russian, the latter Chinese,—in which the chief exchanges of the two empires are effected. After returning to Irkootsk, he went on to Chetah, visited the celebrated mines of Nerchinsk and then proceeded to Chilkah, from which place, in a small row-boat, with two or three companions, beside a crew of five men, he followed the Amoor to its mouth. This river voyage of about 2600 miles he made in fifty two days.

The impressions of Mr. Collins are favorable in every respect to the introduction of American commerce. The river is said to be navigable by steamboats from the junction of the Schilka and Argoon to its mouth. The neighboring country is thickly settled, various articles of export, especially furs and hides, are abundant, and manufactured goods are in demand. Many of Mr. Collins's incidental remarks in respect to agriculture and mining are of an interesting character, but his report must be looked at as a collection of "Observations" rather than as the result of "Explorations." Coming as it does from an American it may serve to draw attention to the much more elaborate and satisfactory investigations which have been conducted during the last few years under the direction of the Russian government.

Before proceeding to enumerate the more important of these expeditions and their several characteristics we stop to inquire the occasion of the impulse lately given to Siberian explorations.

The immense capacity of Russia for producing raw materials has long demanded freer communication between the interior and the coast, both in the east and in the west, than has hitherto been enjoyed. The complete control by the Czar of the Amoor river would have almost as much influence on the development of Siberian resources as the control of the Dardanelles on the prosperity of Russia proper. Let the navigation of the stream be made easy, and a few lines of railway established, and the empire of Russia will be as open as any country to the trade of China, Japan, the East Indies and America.

Taking advantage of the unsettled condition of the country around the Amoor, Russia has been for some years quietly pushing her out-posts farther and farther into the proper dominions of China. Precisely what has been her progress and what are now the claims of her "manifest destiny," can only be learned in the cabinet of St. Petersburg. This much is evident. By the treaty of Russia with China in 1689,* after the well known defeat

* Cf. Petermann on the Amur Stream. Geogr. Mittheil, 1856. p. 472.

at Albasin, and also by the treaty of 1727, the boundary between the two empires was the Northern watershed of the Amoor river on the Stanovoi mountains, leaving the entire basin of the river, east of the junction of the Schilka and Argoon in possession of the Chinese. It was so delineated on the official charts of Russia. In 1844–5, von Middendorf, under instructions from the Imp. Acad. of Sciences in St. Petersburgh, proceeded to determine on the spot the exact boundary line. He reported that the Chinese did not claim as far north as the watershed, but only as far on the left bank of the Amoor as the tributaries were navigable for small boats. Without further ceremony, so far as it appears, some fifty thousand square versts on the south of the Stanovoi summits were accordingly indicated on the charts as belonging to Russian dominion.

The Crimean war caused the removal of large bodies of troops, often under highly intelligent officers, quite to the Pacific coast, for the defence of such Russian possessions as were threatened by the allied fleets. About that time not less than five Russian forts were "provisionally" established on the Amoor, between Ust-Strelotschnaja and the mouth of the Sungari. Nicolaieff, at the mouth of the Amoor river, was fortified, and even so far south (on the right bank of the Amoor) as De Castries bay the Russian flag was raised and a fort erected. The actual possession of the Amoor was thus completed. By recent advices it appears that Russia, in addition to a commercial treaty like that of the other powers, has obtained a treaty conceding to it all of the Amoor territory which had thus been occupied. What are the exact limits of this concession we are not yet informed.

There can be no question that Russia will employ to its own advantage the aggrandizements thus made, but whether its next advances will be in China, or Japan or the English possessions in India the future will reveal. It will not be forgotten that Russia was the first power which watched the movements of Com. Perry in Japan, nor on the other hand that Japan has long been suspicious of its Muscovite neighbor, having even dispatched a special agent to the Amoor river to discover if possible the ulterior purpose of the movements in that region.*

The considerations which we have now presented sufficiently explain the recent energy which Russia has displayed in the explorations of the Amoor. So many scientific investigators have now visited that country that we may anticipate at an early day vast accessions to our knowledge of Eastern Asia. Already the outlines which have been communicated to the world, and which may be found for the most part in the comprehensive "Mittheilungen" of Dr. Petermann, are sufficient to awaken a profound

* Perry's Japan. i. 32.

interest.* It would not be difficult to compile from them a far more reliable and complete report of the Amoor country than could be derived from the observations of many non-scientific travellers.

We proceed to specify some of the accounts of the Russian explorations.

1. We mention first a Report of Peschtschuroff, who accompanied Count Putiatin around Cape Horn to Japan and afterward returned by way of Irkootsk to St. Petersburgh. His astronomical determinations of twenty-three points on the River Amoor, between its mouth and its commencement were published two years ago. More recently he has furnished a hydrographic description of the upper portion of the river from Ust-Strelotschnaja to the mouth of the Sungari, with some general statements in respect to the characteristics of the whole stream and many interesting ethnographical remarks. He refers in this report to a special chart of the Amoor in twelve sheets on a scale of two miles to an English inch (about 1 : 146000) which has been constructed from the sketches of Lieut. Popoff. Dr. Petermann publishes what he supposes to be a reduction of this chart, adding however some corrections obviously demanded by Peschtschuroff's data. The observations now making by Lieut. Roskoff may be expected to give still further accuracy to these delineations. The fact is not concealed that there are obstructions to navigation, especially numerous islands.

2. Herr Permikin has reported to the Imp. Geograph. Society of Russia in respect to the Geology and Natural History of the entire stream. Between the Sungari and the mouth of the Amoor, his journal may be considered in its general statements as supplemental to that of Peschtschuroff.

3. Leopold Schrenk's report to the Academy of Sciences gives an outline of his journey down and up the whole course of the Amoor. He has made important collections for scientific purposes. Thirty of his boxes containing specimens in Natural History, were at the date of recent advices on their way to St. Petersburgh. His investigations of the island Sachalen and his examination of the region around the mouth of the stream, and as far up as the Ussuri are particularly extended.

4. Since 1855, the Imperial Geographical Society of Russia has sustained a party of explorers known as the "East Siberian Expedition" under the direction of the astronomer Schwarz. Its

* The more important of these articles, extending through four volumes of the Mittheilungen (Gotha, 1855–8), are the following :—

1856, p. 175. Latest Russian Acquisitions in the Chinese Territory; by Dr. Petermann. p. 472. Peschtschuroff's Surveys on the Amoor; by the same. 1857, p. 296. The Amoor Stream; by the same. p. 518. L. Schrenk's Latest Researches. 1858, p. 70. Maximowitsch's Researches on the Amoor.

original design was to survey in three years Trans Baikal, and the district between the upper Lena and the Wittim. But the investigations having extended on one side to the Amoor and having been impeded on the other, the party was to continue in the field during 1858. Notices of the progress of the expedition by Dr. Schirren have been printed from time to time in the Berlin "Zeitschrift für allgemeine Erdkunde." The May number of that journal contains a brief summary of the labors of each member of the party during the last three years. Lieut. Roshkoff has been especially charged with the Amoor survey. Radde has been making Natural History investigations in part of its neighborhood.

Roshkoff determined the position of twenty-one places and surveyed the route from Ust-Strelotschnaja to Albasin and from Marien station to Nicolaieff, and the part of the river between his surveys were surveyed by Lieut. Sondhagen.

5. Around the mouth of the Amoor river, and for a considerable distance upon the coast to the North and South, the Russian flotilla has collected much information of importance in navigation. Some of these details are given in the report of the Russian Admirals Sawojka and Putiatin in respect to the naval operations in the straits of Tartary, in 1855, which is published in the official journal of the Ministry of the Marine.

The English cruisers made at the same period soundings and observations, especially around the island of Sachalen, many of which are given in Whitingham's Notes on the late expedition against the Russian Siberia Settlements. (Lond. 1856.)

6. The last investigations to which we call attention are those of Maximowitsch, who has been travelling for the Imperial Botanical Garden of St. Petersburgh. His letters have been published in the bulletin of that establishment, and many of the plants which he collected described by Ruprecht. Maximowitsch travelled part of the time with Schrenk, and part of the time independently.

From this brief indication of the explorations which have lately been made under the patronage or direction of the Russian government, it is evident that we may look for vast accessions at an early day to our knowledge of the Amoor country. All its characteristics have been so little known heretofore, that much has now been unquestionably gained in different departments of natural science. The commercial world will not be long in appropriating the important results of these various investigations.

New Maps of Tropical America.—The German publishers have recently issued three important maps of different parts of Tropical America based upon recent surveys and authentic travels.

The most complete of these charts is Kiepert's "New Map of Central America," published in four sheets, Berlin, Reimer. It includes the territory between 76° and 79° long. west from Greenwich, and between 6° and 22° N. lat. Although immense districts within these limits are unexplored, this map will be found of very great value, as an accurate presentation of what is known. The scale is 1 : 2,000,000. Upon it, there are five subordinate maps. 1. The state of San Salvador, and the proposed Honduras Rail Road from the surveys of E. G. Squier and W. H. Jeffers, in 1853. Scale, 1 : 1,000,000. 2. The isthmus of Tehuantepec as surveyed for a proposed railway, in 1851, by Col. Barnard. Scale 1 : 1,000,000. 3. The river San Juan de Nicaragua, from the survey in 1847, published in 1851 by A. von Bulow. Scale 1 : 500,000. 4. Isthmus and Rail Road of Panama, from the surveys in 1849 by Col. C. W. Hughes. Scale 1 : 400,000. Tract of the proposed Inter-oceanic Canal of the river Atrato, from the surveys of W. Kennish in 1854. Scale 1 : 400,000.

In reference to the materials used in constructing this map, the following note is given by the compiler.

"The coast lines are copied from the British Admiralty charts, surveyed on the Atlantic side principally by Capt. Owen, on the Pacific side from New Granada westward to Point Herradura in Costarica by Capt. Kellett. The part not yet entirely surveyed by the British Navy, from that point to the Isthmus of Tehuantepec has been retained from the old Spanish charts, with some corrections in the principal bays and harbors, made by British French and American seamen.

"Of the interior no part has hitherto been satisfactorily surveyed, with the only exception of the measurements executed for the proposed canal and railroad lines and their next environs on the isthmuses of Tehuantepec, Comayagua, Lake Nicaragua and Panama, specified with the names of the authors on the cartons accompanying this map. The other parts of the map on whose accuracy most confidence may be placed are the following: 1, the part of the New Granadian territory east of the 80th degree, reduced from a map compiled with the aid of old Spanish documents, and corrected by some new surveys by Col. Augustino Codazzi, (published in Berlin, 1857); 2, the central or cultivated part of the state of Costarica, taken from a MSS. map by Mr. Alexander von Bülow; 3, the greater part of the states S. Salvador, Honduras and Nicaragua, copied after the surveys executed in 1851–53 by Messrs. Squier, Jeffers and Hitchcock; 4, the northern part of Yucatan, from a Spanish map, corrected by personal observation and published by Mr. Heller, an Austrian naturalist in 1848.

"To the same gentleman we are indebted for the new intelligence produced on the southern part of the state of Tabasco. All available works of other distinguished travellers (viz. Messrs. Thompson in 1825, Dunn 1827, Legh Page 1834, Montgomery 1838, Stephens 1838–39, Dunlop 1844–46, Wagner and Scherzer 1853–54, and others,) have been consulted in order to correct and to complete the other parts of the map, especially the state of Guatemala the rest of the detail being taken with the necessary precaution in the drawing of the mountains, from the well known but not entirely authentic map of Mr. Baily."

A second chart in two sheets, covering part of the territory included in the map just mentioned, has also been published by Reimer. It is entitled "Carte de l'Isthme de Panama et de Darien, et de la province de Choco." It is based on the surveys of Augustin Codazzi, Colonel in the New Granada corps of Engineers, and is edited by Dr. Kiepert. It is printed on two sheets, on a scale of 1 : 800,000. Subordinate maps (corresponding with those above mentioned) of the routes of the Panama Rail Road and the proposed Atrate canal are also given. In the outlines of the coast, Mr. Codazzi has based his chart on the surveys of the English engineer, Mr. Kellet, published in 1854, with the introduction, however, of some changes. Dr. Kiepert, who is conscientiously exact in all his publications, is forced in a note which accompanies this map, to express his doubts as to the reliability of the delineations of the interior of the country.

A third map, recently issued by the same editor and publisher, is entitled "Tropical America North of the Equator," and comprises the West Indies, Central America, Mexico, New Granada, and Venezuela. It is composed with the help of all cartographic and literary materials hitherto published. One of the sheets contains a subordinate map of the central part of the Mexican Republic on an enlarged scale. The entire work is executed with great clearness and precision.

Recent Explorations in South Australia.—We condense from the Berlin "Zeitschrift für allgemeine Erdkunde" for August, 1858, the following interesting notice of late explorations in South Australia. In the year 1857 two explorations were made under the auspices of the Colonial government into the northern part of the mountainous region which extends from Spencer Gulf north-easterly and nearly to the bottom of the vast curve formed by Lake Torrens. This singular lake, now supposed to consist of immense morasses, salt pools, and shallow expanses of fresh water—so shallow as to be dry during a portion of the year—appears to commence to the northward of Spencer Gulf, (with which it is partially connected by a valley of towards 400 yards in length,) and, extending northerly three degrees, to sweep N.E., E., S.E., then southerly in 140 E. long. to a point

nearly opposite the place of commencing. The mountain land above alluded to, as enclosed by Lake Torrens, consists of the Flinders range, which runs in a nearly straight and unbroken line or belt from the vicinity of Port Augusta north-easterly to 30° 40′ S. lat., and of the Pound Range, etc., a series of detached peaks or spurs, appearing to branch off in all directions from the northern extremity of the Flinders Range. The main object of these explorations seems to have been to lay open new and desirable grazing lands, and thus to direct intelligently the course of colonization. Accordingly Goyder, who made the first exploration in May and June, especially noted vegetation, followed the course of streams, fixed the position of fresh-water springs or pools, and when he reached the south shore of the bend of Lake Torrens in 29° 13′ S. lat. joyfully reports it "as an apparently interminable body of *fresh* water flowing with a decided current towards the northwest." He describes the north shore, as seen through a telescope, as covered with vegetation, and yet he makes no mention of the depth, real or apparent, of the water. As we shall afterwards see, the government was misled into the conclusion that this portion of the lake was navigable, and that it might be made the highway to unexplored wealth in the heart of the continent. Nor was this the only point in which his report proved an unsafe guide. His glowing pictures of the fertility of large portions of the soil were based partly on near at hand observations during the most favorable period of the year when the streams were full, and partly also on bird's-eye views, from the summits of mountains, of wide reaches of landscape invested with the deceitful colors of the *mirage*.

Freeling's expedition, undertaken in consequence of Goyder's report, and in the following September, was chiefly directed to the navigation of Lake Torrens from the point where Goyder had observed it "flowing in a northwesterly direction." For this purpose he was provided with a small iron boat. On his arrival at the above named point, Freeling to his surprise found the water to have receded more than half a mile. The soil thus laid bare was "clay, mixed with sand, without stones;" so too the shore for a mile inland, bore the same character, but arid and cracked into fissures by the heat of the sun. From the very slight elevation of the shore, which bore the appearance of sandy flats, as well as from drift-wood and water-marks, it seemed probable that the lake had already receded some six miles, even at the time of Goyder's visit. Freeling was at once convinced that the lake was not navigable, but he resolved not to return until baffled by actual experiment. He accordingly made three successive attempts to reach deep water. On the preliminary trial he waded but a short distance out, sank ankle-deep in mud

overflowed with water only one inch deep. He then had the boat brought, and this, though on other occasions a light burden for two men, was now, in consequence of the mud, carried with difficulty by six, one quarter of a mile, where finding only two inches of water, they returned. The final experiment was extremely dangerous. The party waded knee-deep through viscid and tenacious mud, three miles from the shore, and found but six inches of water. They were much fatigued with the labor, and under constant apprehension at every step of sinking in some treacherous quagmire, and were rejoiced at their "good fortune in coming upon two small islands, raised but little above the general level, where they rested before retracing their course." Two of the party, however, more courageous than the rest, pushed on for the north shore, thinking to wade across the lake. Their hardihood had nearly proved fatal, for after accomplishing about two miles farther, they became so exhausted, that it was only by the utmost labor that they were enabled to rejoin their comrades. They reported the water somewhat deeper and the mud slightly more yielding. Thus ended the expectations of the government in that quarter. Freeling pronounces the appearance of the lake, its islands, and the opposite shore as seen by Goyder to be due to the *mirage*.

The expedition of Stephen Hack in the summer of 1857 from Streaky Bay to Lake Gairdner and its vicinity, has proved of far greater practical importance than either of the two already noticed. "The new grazing lands discovered to the south of Lake Gairdner comprise an area of more than 4,500 square miles." Hack skirted the south shore of this great lake, but for various sufficient reasons he was obliged to discontinue his explorations, and he returned across the country to Port Augusta. Mr. Harris, surveyor to the expedition, took by azimuth observations, combined with determinations of latitude, the position of all permanent bodies of fresh water, and of the mountains passed on the route, and charted the outlines of the lake-shore by the results of trigonometrical measurements. Mr. Hack's original intention was to have rounded the sourthernmost bay of Lake Gairdner and ascertained its entire outlines upon the east. He thinks there is reason to suspect some union between Lake Torrens and Lake Gairdner, and perhaps that they are one and the same great expanse. It is hinted that the geographical notes of this expedition are of great interest and importance, but they have not yet come to hand. Meanwhile we may judge of the pressing need in South Australia of more extended pasturage, by the fact that within one week after Mr. Hack's return, negotiations were pending for the purchase of about 2000 of the 4,500 square miles of meadow land newly discovered by him. And shortly previous to this, one of the largest proprietors had

been compelled to send a herd of his cattle to New South Wales to graze.

An expedition in charge of Mr. Swinden to explore the region to the west of Lake Torrens left Port Augusta in August, 1857; but the notes we have of it are short and unsatisfactory, amounting to little more than the bare mention of distances between one creek, pool, or spring, and another, and of the character of the water in each, whether brackish or fresh. The great number of such bodies of water, and their nearness to each other, have excited much interest respecting this region, and the reader will doubtless be pleased to learn that a strongly equipped expedition is probably now on the ground, and that the vigorous prosecution of the instructions which Mr. B. Herschell Babbage, its leader, received from the government in February, 1858, will soon result in an accurate knowledge of this now unknown territory. We may add that there are accompanying this expedition, not only a surveyor, but a chemist and a botanist.

Height of the Himalayan Peaks.—The survey, now in progress in Caschmir and Thibet, under the direction of Col. A. S. Waugh, has lately determined the height of one of the peaks of Kara-Korum, and ascertained it to be 27,928 English feet, more than 1000 feet above the Dhaulagiri, and therefore the third in height of all the peaks in the world yet measured.

The following measurements are given for the highest Himalayan peaks:

Mount Everest	29,002	English feet.
Kintschindjunga	28,156	"
Kara-Korum	27,928	"
Dhaulagiri	26,826	"
Tschumalari	23,946	"

Guyot's Physical Tables.—Upon another page of this number a detailed account is given of the Meteorological and Physical Tables, prepared with the greatest care, by Prof. Arnold Guyot, of which a second enlarged edition has recently been published (Washington, Smithsonian Institution, 1858). We allude to the subject here for the sake of calling the attention of those who are interested in geographical investigation, to a variety of tables which they will find of great convenience and value.

D. C. G.

Yale College Library, Dec. 4, 1858.

ART. XI.—*Biographical Notice of Dean Conybeare and Alcide D'Orbigny;* by Major General PORTLOCK, President of the Geological Society of London.*

1. DEAN CONYBEARE.

IT has been justly said of Dean Conybeare that he was one of a *race* of clergymen, and those, men of intellectual eminence. His grandfather was Dean of Christchurch and Bishop of Bristol, the friend of Bishop Berkely, and the author of a work distinguished even in an age of deep thinkers and profound theologians, entitled, "The Defence of Revealed Religion." The Bishop's only son, Dr. Willian Conybeare, Rector of Bishopsgate, left behind him two sons, both of whom were eminent men. The elder, John Josias, Vicar of Bath Easton, was an accomplished scholar, no inconsiderable chemist, a sound geologist, and filled with credit the University offices of Professor of Poetry and of Anglo-Saxon, as well as that of Bampton Lecturer: he promoted the revival of Saxon literature, and left behind him, on his death in early life, a volume of translations which it was his brother's office to complete and edit. That brother, the second son of Dr. William Conybeare, was the illustrious object of this notice, William Daniel Conybeare: he was born in June 1787, and in due time sent to Westminister School, where he received his early education. From Westminister he proceeded to Oxford, and entered Christ Church in the same year as his fellow collegian Sir Robert Peel, taking a first class in classics, in which he was classed with Sir Robert, and a second class in mathematics, in which he was classed with Archbishop Whately. Until he took his M.A. degree, he continued to reside at the University, pursuing various studies, and assisting by his exertions to lay the foundation of geology, which was then only a rising science. At the early portion of the present century, an indifference, such as we can now scarcely understand, as to the cultivation of the natural sciences prevailed at Oxford; but, in the midst of the consequent general neglect, a small band of individuals, residents of the University, were united in the effort to keep alive a taste for at least one branch of natural science, and succeeded in enlisting others in its cause.

The first lectures given at Oxford on Mineralogy, which was then as a study not accurately distinguished from Geology, were, it is believed, those delivered by Sir Christopher Pegge, then Regius Professor of Medicine; and although it may not be possible, either from written records or from the personal testimony

* From the Anniversary Address of the President of the Geological Society of London, Feb. 19, 1858. Quart. Jour. Geol. Soc., vol. xiv, Part 3.

of any one now living, to form an accurate opinion of the merits of those lectures, it may be fairly assumed that they were not destitute of attractiveness, as the same individual delivered long afterwards lectures on Anatomy, remarkable for an elegance and a fluency of diction which have caused them to continue fresh in the recollection of many. Sir Christopher Pegge was succeeded by Dr. Kidd, who for several years gave courses of lectures at Oxford on both the allied sciences, Mineralogy and Geology, and collected around him a knot of persons interested in similar pursuits, who formed themselves into a little club of Oxford Geologists. This club included amongst its members the late Dr. Buckland, the two brothers Conybeare, the late Rev. Philip Serle, of Trinity College, afterwards Rector of Addington, Oxford, and many others, who, though less vigorously devoting themselves to geological research, were still, from their eminent qualities and high character, most instrumental in keeping alive the growing interest for the new science, and in raising the character of the club so high, that some of the early members of the Geological Society of London, then in its infancy, amongst whom were the late Mr. Greenough and the present patriarch of our science, Dr. Fitton, were in the habit of paying an annual visit in Whitsunweek to the University, in order to explore, under the guidance of the geologists of Oxford, the physical structure of the rocks in its neighborhood; whilst, on their part, they thus judiciously enlisted local inquirers in the service of general geology.

The venerable Principal of Magdalen College, Dr. Macbride, is the only survivor at Oxford, of this memorable club, and he preserves at an advanced age the vigor of his faculties, and exhibits all his former interest in the progress of learning and of science; but of non-residents, there still survive Archdeacon Hony, now Prebendary of Sarum, and Mr. Philip Duncan, who now resides at Bath: the latter and his brother, Mr. John Grant, were Fellows of New College, were honored by the degree of D.C.L., and were remarkable not only for their love of natural history, but for their zealous support of every philanthropic and scientific object. The Rev. William D. Conybeare was, however, in the first rank of this little body, and stood so high in the estimation of all its members, that Dr. Buckland, when first lecturing as the successor to Dr. Kidd, expressed in the warmest terms his sense of the obligations he owed to him for the information he had imparted on points relating to geology, and his persuasion that it would not have been fitting for him to offer himself to fill the office of lecturer on that subject, had Mr. Conybeare been desirous to occupy it. Let me add here, that another equally eminent individual, the founder of the new school of geology at Cambridge, as Dr. Buckland was of that of

Oxford, has assured me, with a similar frankness, so characteristic of Prof. Sedgwick, that he too looked upon Dean Conybeare as his early master in geology.

In 1814 Mr. Conybeare married, and retired from the University, the scene of his early triumphs, to undertake the quiet work of a country curacy, and nine years afterwards removed to the vicarage of Sully in Glamorganshire, on the presentation of the late Evan Thomas, Esq., his brother-in-law; but, whilst holding the curacy of Banbury and Lectureship of Brislington, near Bristol, he was mainly instrumental, in conjunction with Sir Henry DelaBeche, in founding the Bristol Philosophical Institution and Museum, and it was at that time he received a visit from the great French geologists, M. Elie de Beaumont and M. Dufrénoy, who came for the purpose of acquiring a knowledge of the secondary rocks of England, as a standard of reference for those of France; and he so deeply impressed them, whilst acting as their companion and guide in an exploration of the neighborhood, with a sense of his geological knowledge, that they were prepared on their return to coöperate with Cuvier in obtaining the election of Mr. Conybeare as a corresponding member of the Institute for Geology. Nor must it be supposed that this excellent man neglected his sacred duties whilst storing his mind with the richest treasures of geological research, as it was during his residence at Sully that he delivered, gratuitously, at the request of his friend Dr. Prichard, a course of theological lectures at Bristol College, of which institution he had become a visitor.

In 1836 he left Sully and went to Devonshire, having presented himself to his family living of Axminster, and, whilst there, preached, at the request of the authorities of the University of Oxford, the Bampton Lecture for 1839. The living of Axminster he resigned after a few years, on being called by his friend Bishop Copleston to the care of the Cathedral of Llandaff. Here he continued zealously to carry on the good work of restoration which had been commenced by his predecessor Dean Bruce Knight; and, as at all times in his life, he was ever ready to distribute the rich and varied stores of his mind for the benefit of his fellow-men in whatsoever station of life they might have been. This venerable, much-loved man, and admired philosopher, left Llandaff to attend the death-bed of his eldest son, and, whilst pausing in his return at the house of another son, was stricken with pulmonary apoplexy, and died on the morning of the 12th of August, after an illness of only three hours, in the 71st year of his age.

Such is the general picture of the life of a truly estimable man; and I shall now add to it a very brief notice of his most characteristic works, premising, however, that, even before the

peace of 1815 had opened the Continent to British geologists, Mr. Conybeare had formed, from the imperfect data then within his reach, a sound opinion as to the identity of the Jura limestone with the oolitic formations of England, an anticipation which he had afterwards the gratification, in conjunction with Dr. Buckland and Mr. Greenough, of verifying. The versatility of the genius of Dean Conybeare led him to examine and describe the lesser points connected with organic remains, as well as the greater; a circumstance in which he strongly resembled his friend and fellow-laborer, Dr. Buckland. For an exemplification of this peculiarity of his mind, I shall refer to his paper published in the year 1844, in the second volume of the Transactions of the Society, and therefore one of his early contributions to palæontological science. It was entitled, "On the Origin of a remarkable Class of Organic Impressions occurring in nodules of flint." Mr. Parkinson had described them as "small round compressed bodies, not exceeding the eighth of an inch in their longest diameters, and horizontally disposed, connected by processes nearly of the fineness of a hair, which pass from different parts of each of these bodies, and are attached to the surrounding ones; the whole of these bodies being thus held in connexion." Mr. Parkinson considered that these bodies were the works of polyps, and he therefore classed them with corals of some unknown genera; and Dr. Buckland, who had directed his attention to them simultaneously with Mr. Conybeare, considered that the moulds in which the siliceous casts had been formed were the work of parasitic insects, the thin hair-like appendages having been the passages of entry first made by the insects, and the larger flattened bodies the cavities afterwards excavated, the object of the excavation having of course been to obtain nourishment from the body thus eaten into, whether a shell or any other. This observation of Dr. Buckland was communicated to Mr. Conybeare, but not until he had completed his own researches, and arrived at the same virtual conclusion,—namely, that "these cellules were the works of animalcules preying on shells and on the vermes inhabiting them." In arriving at this conclusion, Mr. Conybeare was guided by the examination of various fragments of shells, still preserved in contact with the siliceous matter which had subsequently been infiltrated into the cavities produced by the boring animal. These appear to have been portions of shells distinguished by a striated texture, and were stated by Mr. Conybeare to resemble in structure the recent *Pinna marina*, as the genus *Inoceramus* does; but in addition to these, Mr. Conybeare found them connected with other shells, and even with an *Echinus* and *Belemnite*. Though Mr. Conybeare spoke with diffidence of his having brought before the Society a paper on such minute palæontology, it cannot

be doubted that the interest connected with the discovery of the existence and workings of minute marine animals at so remote an epoch is of a very high order. The flints and other siliceous deposits of the chalk and other geological epochs, have indeed been striking examples of the effect of judicious investigation in rendering the most obscure objects the means of throwing light upon natural phenomena.

Mr. Conybeare was fully aware of the necessity of studying physical as well as organic phenomena in connexion with geological science; and it is truly surprising how often the intimate connexion of the physical geography of remote epochs with their natural history is overlooked. His description of the landslip which occurred on the coast of Culverhole Point, near Axmouth, in December 1839, was ably illustrated by a series of lithographic plates from the drawings of the present Lieut. Colonel Dawson; and the magnitude of the results was well expressed by the following words: "Although this convulsion can only be ascribed to the less dignified agency of the land-springs constantly undermining the sub-strata, yet, in the grandeur of the disturbances it has occasioned, it far exceeds the ravages of the earthquakes of Calabria, and almost rivals the vast volcanic fissures of the Val del Bove on the flanks of Etna." Without doubt these phenomena are very striking and interesting in themselves: but they become still more so when we reflect as Mr. Robert Mallet has taught us to do, that they ought not to be confined to the existing epoch alone, but should be sought for in the stony records of past ages. The paper on the Hydrographical Basin of the Thames, written with a view to determine the causes which had operated in forming the Valleys of the Thames and its tributary streams, is equally valuable as tending to maintain the value of attending to physical geography in geological investigations. His examination, also, of the Theory of Mountain chains, then recently propounded by M. Elie de Beaumont, as well as his remarks on the phenomena of geology which most directly bear on theoretical speculations, are proofs of the truly philosophical and enlarged view he took of his favorite science.

In noticing the works of Dr. Buckland, I have already detailed the importance of the paper which was compiled by him in conjunction with Mr. Conybeare, on the Bristol and South Welsh Coal-fields; one, as I then observed, of those elaborate and comprehensive papers which were the fitting work of the first pioneers of geological science, and the difficulty of which can scarcely be appreciated in these times when the foundations of the science have been fairly laid, and geologists have only to improve or correct the details. His remarks on the sections of the Antrim and Derry coast were also a conjoint work, and of much interest.

Another and equally remarkable work was that undertaken in conjunction with the late Mr. William Philips, namely, the "Outlines of the Geology of England and Wales," as it may be considered the first systematic work on the subject; and, though geology has been since more specialized and studied in minuter detail, this work will always be regarded as a striking proof of the ability and knowledge of the authors.

It was, however, in 1821 (April 6) that Mr. Conybeare communicated to the Society that remarkable Palæontological paper which excited so much interest at the time, and established in the most satisfactory manner the propriety of establishing a new genus of *Reptilia*, forming an intermediate link between the *Ichthyosaurus* and Crocodile, to which Mr. Conybeare gave the name of *Plesiosaurus*.

The discovery of immense vertebræ of oviparous quadrupeds in the Lias near Bristol had attracted the attention of Mr. Conybeare, who quickly recognized the difference between those belonging to the *Ichthyosaurus* and others, which evidently in his opinion were portions of a different animal. With a singular acumen and rare sagacity, he placed the detached vertebræ in their proper position, and finally established his new genus, for which he adopted the name *Plesiosaurus*, as expressing its near approach to the order Lacerta.

For the whole group of animals which approximate, on the one hand, to the crocodiles in general organization, and yet have been provided with such specific organs as were necessary to enable them to live, at least principally, in the sea, Mr. Conybeare proposed the name *Enaliosauri*, as a classic appellation for the whole order; and he observes of the genera composing it, that even the *Ichthyosaurus*, which recedes most widely from the forms of the Lizard family, and approaches nearest to those of fishes, exhibits in its osteology a beautiful series of analogies with that of the crocodile, and which widely remove it from fishes.

In this paper he then described in the minutest detail the osteology of the *Ichthyosaurus*, and exhibited a knowledge of anatomy which excited the admiration of every one. He then examined with equal care the relics of the new genus, which, although at that time not complete, were sufficient to enable Mr. Conybeare to conclude that the vertebral column recedes from that of the *Ichthyosaurus* in all the points in which the latter approaches to the fishy structure, and that the invertebral substance must have been disposed much as in *Cetacea;* and that, from the locking together of the articulating processes, it must have had much less flexibility than in the *Ichthyosaurus* or in fishes. In examining also such portions of the paddles as could be arranged in order, he comes to a similar conclusion in another direction, namely, that the paddles of the *Plesiosaurus* are inter-

mediate in character between those of the *Ichthyosaurus* and the Sea-turtles; and thus in every respect he laid a sound foundation for his new genus.

It is to be remarked that this paper was given as the joint production of Mr. Conybeare and Sir Henry De la Beche, to whom Mr. Conybeare most liberally ascribed a full share of the merit of the discovery; but, allowing Sir Henry every praise for his assistance in that discovery and in all the geological details, I believe the sagacity and skill exhibited in the osteological details and reasonings have always been ascribed to Mr. Conybeare.

In a second paper, read May 3, 1822, Mr. Conybeare was enabled to describe much more fully all the relations of the genera *Ichthyosaurus* and *Plesiosaurus*, from the discovery of other remains, both of the *Ichthyosaurus* and *Plesiosaurus*, by his coadjutor Sir Henry De la Beche. A very minute examination of the teeth, especially, enabled him to point out that those of the *Icthyosaurus* were more intimately related to the teeth of the crocodile than to those of other *Lacertæ* (an opinion then at variance with the opinions of some anatomists), whilst at the same time, in other respects, the analogy was in the other direction, for Conybeare observes, "in pursuing, however, the history of the teeth of the *Ichthyosaurus* to the last stage, we quit these analogies with the crocodile, and arrive at another point wherein the *Ichthyosaurus* resembles the other *Lacertæ*, in common with many of the *Mammalia:* this is the gradual obliteration of the interior cavity in old age, by the ossification of the pulpy nucleus." In conjunction with Sir H. De la Beche he brought up the number of species to four, determined from the teeth; and in his further consideration of the genus it is right to notice the following remarks, proceeding from him after noticing a difference in one character of the fossil crocodile, when compared with the recent, as stated by Cuvier:—"I am persuaded from every circumstance, that a much nearer approximation to the structure of the older lacertian genera will be found in the fossil than in the recent crocodiles: interesting links in the chain of Saurian animals will be thus supplied, and it will probably be found that many of the points in which the *Ichthyosaurus* differs from the recent type are only instances of its agreement with the fossil."

The researches of Sir H. De la Beche had not at this time led to the discovery of a complete skeleton of the new genus *Plesiosaurus;* but additional portions of it were found, including a very perfect dental bone of the lower jaw, whilst a tolerably perfect head was discovered by Mr. Thomas Clarke in the Lias of Street, near Glastonbury.

The investigation of these new relics of the *Plesiosaurus* led Mr. Conybeare to the following conclusion: "On the whole then, the manner in which the ribs of the *Plesiosaurus* articulate

throughout, by a single head, to the extremity of the transverse processes of the vertebræ only, the structure of the humero-sternal parts, and the characters derived from the head, approximate this animal most nearly to the *Lacertæ*. By its teeth, on the other hand, it is allied to the crocodile; while its small nostrils and multarticulate paddles are features in which it resembles the *Ichthyosaurus*." This able paper he concluded with words characteristic of his natural modesty, after pointing out the difficulty of rendering anatomical details at once scientifically accurate and yet attractive to a general audience: "I need not add how much these difficulties will be increased in the hands of a writer who must acknowledge that, while intruding on the province of the comparative anatomist, he stands on foreign ground, and, using almost a foreign language, is frequently driven to adopt an awkward periphrasis, where a single word from the pen of a master would probably have been sufficient."

However some may at the time have been inclined to throw doubts upon the deductions of Conybeare, the ability and accurate discrimination of the author were publicly recognized by the great Cuvier, who hastened to advocate his admission to the French Academy as a Corresponding Member for the science of Geology; and I am sure that all living palæontologists will follow the example of the late well-known, and at that time so highly respected, Mr. Clift, in recognizing the great merits of Dean Conybeare, and considering him one of the principal founders of the science in this country.

At the present moment it would be tedious and unnecessary to pass in review the whole of the long series of Mr. Conybeare's geological works, nineteen in number; and I shall point your attention therefore solely to that able "Report on the Progress, Actual State, and Ulterior Prospects of Geological Science," which he presented to the British Association in 1832, at its meeting in Oxford, in which he treats the subject with the combined powers of the scholar and man of science, pointing out the remarkable analogy in the views of Leibnitz to those of many modern speculators on physical geology; the opinions of Hooke in respect to the hypothesis of the elevation of our continents by volcanic agency; the masterly observations of Smith, first made known in 1799, which, although not the first to originate the doctrine of a regular distribution of organic remains, yet reduced to certainty and order what had been before vague and conjectural; the gradual rise of the Tertiary Geology from its foundation in the admirable "Memoir on the Basin of Paris," by Cuvier and Brongniart, published in 1811; the establishment of the Geological Society in 1808, and the labors of all the great men connected with it, including, amongst many others, Greenough, Buckland, Sedgwick, Fitton, Murchison, De la Beche, Phil-

lips, Scrope, Daubeny, and Lyell, together with those of foreign geologists, including the great Von Buch and Boué. That Report alone is sufficient to prove his masterly acquaintance with the history of his favorite science, and with all its bearings, whilst it marks the liberal spirit with which he entered into all geological inquiries. The advance of geology since that Report has been enormous; and, if a period of twenty years from the publication of Cuvier and Brongniart had done so much in raising Tertiary Geology to a high position, may we not say that the result of the next twenty-five years has been still more remarkable, and has richly rewarded the continued and judicious researches of some of our most distinguished geologists, such as Lyell, Forbes, Prestwich, and Austen, whilst the elevation to which the Silurian system has arrived by the persevering exertions of Murchison is a monument of progress which we can scarcely hope will be equalled in that peculiar branch of geology in future times.

The zeal of Dean Conybeare for geology never forsook him; and when obliged to visit Madeira on account of the health of his youngest son, he visited the Peak of Teneriffe, and studied the other volcanic phenomena of the neighboring islands. How deeply must we regret that his last days were embittered by sorrow for the death of another son, from whose funeral he was returning at the time of his death! But so excellent a man, prepared for death by the strict performance of every Christian duty during life, requires not the commiseration of those who survive him; although all who recollect his air of gravity and of sincerity, which always made his words effective in commanding attention and respect, and in bringing home conviction to the minds of his hearers, must feel how heavy a loss we have experienced.

2. Alcide D'Orbigny.

Alcide D'Orbigny, Professor of Palæontology at the Museum of Natural History in the Jardin des Plantes, was remarkable for the vast magnitude, as well as for the interesting character of his palæontological works, intended as they were to embrace the whole field of geology in France, and, of course, comparatively to notice the relations of the ancient inhabitants of all portions of the earth whilst describing those of his native country. Mr. D'Orbigny was born at Couëzon (Loire Inférieure), and was, in succession, Travelling Naturalist for the Museum of Natural History, Secretary of the Natural History Society, Member of the Central Commission of the Geographical Society, Assistant of M. Cordier in the Geological Course, and latterly occupant of the chair of Palæontology which had been created expressly for him. He was a Knight of the Legion of Honor.

Mr. D'Orbigny commenced in 1826 his travels for the Museum, under the auspices of the government. As a student at Rochelle, D'Orbigny passed his earlier years on the sea-shore, and employed much of his time in examining the natural productions thrown ashore by the waves. Before he had attained the age of twenty-two, he presented to the Academy a work which was attended with great success, as the committee appointed to examine it reported that, from the great number of new species he had made known, he deserved to be placed in the first rank of original observers. In 1826 he proceeded, as Travelling Naturalist for the Museum, on a voyage to South America, where he explored, with equal perseverance, courage, knowledge, and success, Brazil, Buenos Ayres, the frontiers of Patagonia, and the Republics of Chili and Bolivia, from the shore of the Pacific Ocean to the centre of the continent: he afterwards went through the Republic of Peru, and, when he returned to France, had visited all that portion of the earth from the 11th to the 12th degree of latitude, and from the Pacific to the Atlantic Ocean.

As the product of this voyage, Mr. D'Orbigny brought home most extensive collections and manuscripts, numerous drawings of objects of natural history, and everything necessary to illustrate the geography, the languages, the ethnology, and archæology of this part of America: historical manuscripts, thirty-six vocabularies of the American language, a collection of animals containing 7000 species, of which many were new, and one of about 2300 species of plants, as well as much information respecting the geology of the countries he visited, were amongst the results of his labors, and were embodied in the great work entitled, "Voyage dans l'Amérique du Sud," published under the sanction of the Minister of Public Instruction. He also superintended the publication of another work, "Voyage pittoresque dans les deux Amériques;" and his labors were appreciated by the Geographical Society of France, which awarded him its annual prize in 1836. As an active, intrepid, and persevering traveller, he had thus made his way over an immense extent of country, from Brazil and Peru to Patagonia, in eight successive years, sometimes navigating previously unknown rivers, sometimes penetrating virgin forests, resting on the loftiest plateaux of the Andes, or in the plains of Patagonia, frequently finding himself amongst contending tribes, and being obliged to take part in their conflicts.

Alcide D'Orbigny, who had thus studied nature under all its varied forms, now devoted himself to a task not less deserving of the admiration of posterity, as he thenceforth consecrated his life to the study of Palæontology, a science which had only sprung into existence in the nineteenth century, and which has

already enabled the geologist to study the ancient natural history of the several epochs of the earth's history, and to determine by that clue the true *relative* age of the mineral deposits with which the fossil relics of animals and plants, long since removed from observation as existing genera and species, are associated. It has been justly said that what he succeeded in accomplishing in this new branch of science, was so vast as to be almost beyond the intelligence, and, I may add, the physical powers of any one man; and, as a proof, I will at present mention his Foraminifera of Cuba, of the Canaries, of Meudon near Paris, and of Vienna; his studies on the Crinoids, his "Prodrome de Paléontologie," his "Course of Stratigraphic Geology," and especially his "Palæontology of France," which has extended to fourteen volumes, and contains 1400 plates of French fossils.

Alcide D'Orbigny was removed by death only four years after he had been chosen Professor at the Jardin des Plantes, and before he had had time to complete his great palæontological works, though it is believed that he has laid the foundation of a palæontological collection worthy of France. I have on a former occasion spoken of the nomenclature introduced by him into geology, which, although founded in great measure upon that previously adopted in England, deserves, from its simplicity, and in many respects its euphony, the ready reception which it has obtained on the continent. In respect to his great work on the Palæontology of France, I am aware that many English palæontologists consider that he has been sometimes too hasty in the creation of new species; but this error, I fear, is common to a large portion of palæontologists, and will not be entirely remedied until naturalists have made their comparisons, not with drawings, but with actual specimens. Making, however, every deduction on that account, the works of Mr. D'Orbigny must ever stand forth as a memorial of the most persevering industry and of a high order of intellect, in confirmation of which opinion I will briefly but more particularly notice some of his numerous works.

In doing so I shall principally confine myself to the notice of such works and opinions of D'Orbigny as affect materially either the philosophy or the practice of geological science. Such papers as his Monograph of the new genus of Gasteropods to which he gave the name *Scissurella*, or his description of two species of the genus *Pteroceras*, found in the Jurassic limestone of La Charente Inférieure, or his essay on the beaks of fossil Cephalopoda, in which he divides the Rhyncholites into two divisions, belonging to different genera, one being the beaks of *Nautili*, and not of *Sepiæ*, as had been before supposed,—an idea supported by the anatomical description, by Professor Owen, of

the *Nautilus Pompilius*,—or his note on the genus *Caprina*, his tabular view of the class Cephalopoda, his memoir upon a second living species of the family of Crinoids, to which he gave the generic name *Holopus*, and many other of his papers, are sufficient proofs of his great knowledge of, and accurate judgment upon, almost all branches of natural history; but others speak the language of a philosopher on such subjects.

Every one will doubtless remember the different opinions which were once entertained on the true position, amongst organized beings, of the Foraminifera, some naturalists having, from the resemblance of form, allotted them to the Cephalopoda. After a careful examination of the animal portion as well as of the shelly covering of these minute, often microscopical, bodies, he disproved the earlier notion of their alliance to the Cephalopods, which he had himself at first adopted, and proposed a general classification of the Foraminifera, founded upon the form of their shells, placing them amongst the Radiata, close to the Polyps. In this great and important inquiry he described and figured 118 new species from the Island of Cuba and from the Antilles, and afterwards 43 species from the Canaries, of which 33 were peculiar to those islands. Nor was it to living Foraminifera that he confined his attention, as he described and figured 54 species from the white chalk of the Paris basin, all, with the exception of three or four, new, and then again those which had been discovered by M. von Hauer in Austria, ending by the following statement of the geological distribution of Foraminifera:

	Genera.	Species.
Palæozoic strata	1	1
Jurassic strata	5	20
Cretaceous strata	34	280
Tertiary strata	56	450
Existing epoch	68	1000

So that it would appear that the genera and species were few in number and simple in structure at first, and increased both in number and complexity of structure from formation to formation, until they had obtained their maximum of development in the present seas. D'Orbigny even considered that this gradual advancement from simple to compound was more distinctly manifested in these minute beings than in any others, and that they are in consequence the best fitted for determining with precision the relative ages of geological strata. The following ten living genera, *Gromia*, *Rimulina*, *Conulina*, *Vertebralina*, *Caudenia*, *Pavonina*, *Robertina*, *Cassidulina*, *Uniloculina*, and *Cruciloculina*, M. D'Orbigny named as not having been as yet discovered in a fossil state; and he gave the following view of the climatal distribution of the Foraminifera, which also cannot fail to be

very suggestive to the palæontologist. Torrid Zone, 375 species; Temperate Zone, 350; Frigid Zone, 75: so that, as in Mollusca, the seas of hot climates are more productive of species of Foraminifera than those of colder regions.

D'Orbigny traces the history of these bodies from their first discovery in 1731 to the present time; and as a proof of the importance of the office they may have played in the formation of some geological strata (the houses of Paris and the pyramids of Egypt being in part built of rocks composed of Foraminiferous shells), he states that little more than an ounce in weight of the sand of the Antilles yielded 480,000 of these shells. D'Orbigny concluded, from his examination of the Foraminifera of the Paris basin, that they had lived in a hot climate, and had not been subjected to the wearing action of any current.

In explaining the distribution of the Foraminifera of South America, Mr. D'Orbigny points out how varied the groups are, under the influence even of chorographic differences,—the Foraminifera of the southern shores of the Pacific differing from those of the southern shores of the Atlantic, and both from those of the equatorial region of the Antilles, from which fact he deduces the conclusions, that in the same sea, and in connexion with the same continent, different fauna may exist at very small distances from each other; and further, that Tertiary basins, although different in their fauna, may have been formed simultaneously, just as the material deposits are necessarily widely different in character at localities by no means very remote. Unquestionably the reasoning is good, and equally applicable to the geological deposits of all ages of the world.*

In his essay on the distribution of the Acetabuliferous Cephalopoda, he states, in reference to their present distribution, that 15 out of 16 genera are found in hot countries, 10 in temperate regions, and 6 only in cold; and he also concludes, from his inquiries, that these forms are more complicated as they inhabit hotter regions, and further, that it is probable the fossil genera

* It must not be assumed from my remarks on D'Orbigny's labors in the *Foraminifera*, that I consider him to have arrived at his final results *per saltum*. Far from it, as in 1826 his object, as so well explained by Férussac, was simply to separate the microscopical *Cephalopoda*, as he then considered them to be, from the Siphoniferous genera with which they had been confounded. De Haan had previously proposed such a separation, and founded upon it his *Siphonoides* and *Asiphonoides*; but D'Orbigny felt that there were other differences, and therefore proposed his more distinctive term *Foraminifera*. His "Prodromus," published at that time, was founded upon this view of the subject, and remained the standard of classification until Desjardins, in 1835, gave many reasons, deduced from careful observation, for separating the *Foraminifera* from the Mollusca entirely, and forming of them a totally distinct class, to which he gave the name *Symplectomères*. Desjardins therefore gave the impulse which has since led to the correct classification of these microscopical but most interesting animals, which have been shown, by the examination of the deep-sea soundings of the Atlantic, to be as active now as in ancient epochs in laying the foundations of future strata.

lived under a high temperature. Taking account of this view of the subject, it is interesting to observe the other statement of M. D'Orbigny, that the Acetabuliferous Cephalopoda appeared first in the Jurassic formation, when they were represented by the *Belemnites* and six other genera, including the existing genus *Sepia* and three other living genera, simultaneously with the vast numbers of *Ammonites;* that all disappeared except the genus *Belemnites* in the Cretaceous epoch, being represented, however, by different species; and that in the Tertiary strata, the *Belemnites* disappeared entirely, being replaced by the genus *Sepia* appearing for the second time, and the genus *Beloptera*, which appeared, only to pass rapidly away, as it is no longer a living genus. These are unquestionably very remarkable facts; and have on the one hand a tendency to support the doctrine which M. D'Orbigny so strongly supports, of the destruction of one creation and the production of another again and again at successive epochs, whilst, on the other, they may induce a pause in the decision of the palæontologist, as it seems difficult to conceive that any such genera as *Sepia*, *Sepioteuthis*, &c., could have been created so far back as the Jurassic age, and then have totally disappeared, to be *again created* in the Tertiary and existing epochs. I must again maintain that it is more natural to conceive that the link of connexion between the dead and the living has been kept up, although hitherto the region of their habitation, during the long period of time elapsed, has been veiled from observation.

I shall not attempt further to follow the able author of no less than fifty distinct treatises, some of vast magnitude and interest, and all full of ingenuity and knowledge; but I may notice him as the author of that nomenclature which is gaining ground rapidly; and in doing so I will quote, as illustrative of his method, the distribution of the Bryozoa-Cellulina, which he thus details:—

		Genera.		Species.	
Terrains Crétacés.	Etage Néocomien ...	1	.....	1	593 species.
	— Aptien	1	.....	1	
	— Albien	1	.....	1	
	— Cénomanien ..	11	.....	26	
	— Turonien	9	.....	17	
	— Sénonien	54	.....	547	
Terrains Tertiares.	Etage Suessonien....	3	.....	5	109
	— Parisien.....	12	.....	24	
	— Falunien	40	.....	75	
	— Subapenin ...	4	.....	5	
Existing Fauna.	.....	58	.....	312..	312

The Bryozoa-Centrifugina, which form the other division of the class, he discovered in almost all the geological formations, and he gives their numbers thus:—

	Genera.	Species.
In the Palæozoic	10	66
—— Triassic	0	0
—— Jurassic	32	93
—— Cretaceous	130	480
—— Tertiary	32	101
—— Existing epoch	26	80

And he concludes from the whole that there were *three centres* of development of the Bryozoa, the first two composed of B. Centrifugina alone,—namely, one in the Carboniferous stage of the Palæozoic, and one in the Bathonian of the Jurassic,—and the other composed of both orders, Cellulina and Centrifugina, in the Senonian stage of the Cretaceous.

Having now, I trust, enabled every one to form a correct judgment of the great and varied abilities of Mr. D'Orbigny, in aid of whose researches the Society has twice awarded the proceeds of the Wollaston Fund, I will close my remarks with the following passage from the report of Messrs. Brongniart, Dufrénoy, and Élie de Beaumont, on his "Geology of South America," as it conveys a sentiment in which all our members will, I am sure, cordially concur:—

"The author's reserve, in treating upon a subject so vast and difficult, cannot but be approved, although no one can fail to perceive that the memoir of M. D'Orbigny has enriched science with a great number of new facts and with many ingenious speculations. New observations may hereafter lead to a modification of some of his theoretical views; but the merit will always be his of having considered a vast subject from a point of observation so elevated as must necessarily cause it to command attention, and lead the way to still further progress. We therefore propose to the Academy that it should express to the author the high satisfaction it has experienced in contemplating the indisputable advancement which has been made towards a knowledge of the geology of South America, by his courageous and persevering researches:"—let me also add, towards a knowledge of the geology of all parts of the earth; for his great works on the Palæontology of France deserve such commendation.

ART. XII.—*Caricography;* by Prof. C. DEWEY.

(Continued from vol. xxiv, p. 48, Second Series.)

No. 254. *Carex Geyeri*, Boott, Lin. Trans., vol. xx, p. 118, Kunze, No. 55. Illus. Car. Boott, No. 98, Tab. 105.

SPICA unica androgyna, superne staminifera cum squamis oblongis obtusis, inferne pistillifera; fructibus 1–4 alternis subremotis tristigmaticis obovatis triquetris ore integris et albis inferne productis vel stipitatis glabris, squamam oblongam magnam amplectentem cristatam subæquantibus; culmis superne foliaceo-bracteatis.

Culm a foot high, slender, sometimes scabrous above, with stiff radical leaves as long as the culm and rough on the edges; spike single, an inch or more long, upper half inch staminate and slender-cylindric with long and oblong close whitish scales; the lower part pistillate, with 1–4 large fruit, which are separate and subremote or scattered, sometimes 4 fruit along an inch, often fewer and nearer, the upper fruit sometimes one-fourth inch below the staminate; stigmas 3; pistillate scale broad, oblong, clasping, more or less awned, and a little longer or shorter than the fruit.

Mountains of North America; C. A. Geyer, in honor of whom it was named; also, Duffield's Ranch, Sierra Nevada; Dr. Bigelow, Explorations for Pacific Railroad, vol. iv, p. 163.

This species is related to *C. phyllostachys*, Meyer, which has a shorter and ovate fruit with a scale very long and leaf-like. Its association is with *C. Wildenovii*, Schk.

No. 255. *C. decidua*, Boott, Lin. Trans., vol. xx. p. 119. Illus. Car. No. 157, p. 63, Tab. 170.

Spicis 3–6 cylindraceis erectis gracilibus atro-purpureis; suprema staminifera brevi-pedunculata clavæ-formi interdum basi vel medio pistillifera, squamis oblongis obtusis vel obovatis doro-albi-nervosis instructa; pistilliferis 1–5 distigmaticis sessilibus bracteatis, superioribus 1–3 brevibus parvis contiguis interdum geminatis apice staminiferis, inferioribus 2 longioribus subremotis foliaceo-bracteatis; fructibus oblongo-ovatis vel obovatis rostellatis inferne teretibus deciduis nervosis ore integris, squama oblonga obtusa dorso pallida subduplo brevioribus.

Culm a foot or more high, triquetrous and quite rough on the edges above and leafy towards the base, and sheathed, rather slender; staminate spike single, short-pedunculate, often pistillate at the base and sometimes in the middle, longish and club-form, with staminate scales oblong, obovate, obtuse and white on the keel; pistillate spikes 1–5, erect, sessile, rough-bracteate,

sometimes 1–3 short approximate to the staminate, and staminate also at their apex, and two longer and remoter below, sometimes only 2 or 3 pistillate spikes of which the upper is near the staminate and the other more remote; stigmas 2; fruit oblong, ovate, or obovate, short rostrate, entire at the orifice, nerved and tapering below, but scarcely stipitate; pistillate scale oblong obtuse pale on the back, and near twice the length of the fruit.

First found in Terra del Fuego; afterwards with the preceding, by Dr. Bigelow, as noticed in the same work. My specimens are from the latter locality.

Note.—As the authority of Willdenow led to the confounding of *C. paleacea*, Wahl., with *C. crinita*, Lam., it is important to say *how* the confusion has been ascertained and the mistake corrected. This has been done by Dr. Boott in his "Illustrations of the Genus Carex," a title so unpretending of a magnificent work on Caricography, finely characterized by Prof. Gray in the July number of this Journal.

Wahlenberg gave a specimen of his *C. paleacea* to Mr. Tuckerman, who passed it to Dr. Boott. It proved to be the *C. maritima*, Vahl., and of course, was very far from *C. crinita*, Lam. But our botanists had long before found a plant, which was figured by Schk. as one form of *C. crinita*, and they were thence led to call another apparent form of it, var. *paleacea*, as being the *C. paleacea*, Wahl.; Schk. supported the same mistake. Having thus corrected the mistake of Willd. and error of Wahl. and of Schk., Dr. Boott saw that our *so-called* var. *paleacea* is the real *C. crinita*, Lam. Hence, the other form of it must be the variety, if it belongs to it. But Dr. Boott shows the manifest difference between them, and gives to this the name *C. gynandra*, appropriated to it by Schweinitz as early as 1824. The true *C. crinita*, Lam. then is ascertained, and another species is named. This and its synonyms will be as follows:—

No. 256. *C. gynandra*, Schw., An. Tab. Boott Illus. Car., No. 48, Tab. 50. *C. crinita*, Schk., fig. 125, Tab. Eee, not of Lam. Dew., Sill. Journ., vol. x, p. 270, and Am. Auth. Var. *gynandra*, Schw., and Tor., and others.

With the change of names in Sill. Journ., vol. x, p. 270–1, as indicated above, the description there is definite and adequate, as shown by the specimens sent by me to Dr. Boott.

I have not often seen the peduncles of the lower spikes so long as on Dr. Boott's figure. The spikes too are generally larger, often somewhat ventricose in the middle, with more staminate flowers at their tapering summit, and more densely fruited than those presented, or more like fig. 125, Schk. The geographical range is greater than shown by Dr. Boott's speci-

mens, and extends over much of New England, New York, and far into Pennsylvania.

The separation, long desired by some, of this species from *C. crinita*, has thus been accomplished.

It is thus made easy to settle the synonyms of *C. crinita*, common in American authors.

C. crinita, Lam. Boot, Illus. Car., No. 47, Tab. 49. Schk., fig. 164, Tab. Ttt. Muh. Gram., p. 229. *C. leonura*, Wahl. Sartwell Exsic. Car., No. 58. Var. *paleacea*, Dew., Sill. Journ., x, p. 270–1. Tor. Mon., 401. Carey and Gray's Manual, p. 549.

Changing the names in Sill. Journ., vol. x, p. 270–1, as already indicated, the description there will distinguish the true *C. crinita*. Dr. Boott's figure fully and finely shows this species, and is far superior to fig. 164, Schk., above mentioned, though I had in 1826 referred this species to it. The long and slender, whipform, densely flowered spikes, with the long and rough-awned pistillate scales, and the roundish or obovate or oval fruit, short-beaked and ventricose, form distinctive characters. They describe *C. crinita*, Lam., there called var. *paleacea*.

Var. *minor*, Boott, as above.

Spikes smaller and shorter, 1–2 inches long, often nodding, or erect, rather loose-flowered, commonly with a long lanceolate and rough-awned scale.

These characters are plain on my specimens of this variety.

This species differs from *C. gynandra*, Schw., in having smooth, and not scabrous sheaths of the leaves, more slender and longer pistillate, spikes more closely fruited, as well as in the fruit and scale.

Dr. Boott's enthusiasm, position and extensive collection of Carices, as well as his acute discrimination, enable him to make other corrections, some of which at least will much interest our students of this vast and difficult genus. I advert to one more, viz., the proper designation of the species so long known over the country as *C. anceps*, Muh. The proper extension of the species is another consideration on which there may be difference of opinion. It was so named by Muhlenberg, in letters to Willdenow, on account of its *two-edged* peduncles of the spikes, and was published by Willd. under that name, though Muh. afterwards published it as *C. plantaginea*, and yet referred it to Schk.'s figure of *C. anceps*. In 1826 I referred it to the synonym, *C. striatula*, Mx., without any consideration of the priority of the name. Several varieties of it were described, and much later Mr. Carey, in Gray's Bot., named one of them (under *C. anceps*) var. *striatula*, very appropriately. In 1857, Dr. Gray, in his Manual of Botany, referred it to *C. laxiflora*, Lam., and, in 1858, Dr. Boott published the reasons for this reference in his Illustra-

tions, with the synonyms and several varieties. There is much propriety in presenting some of the changes, observing at the same time that this species had been called in our country, to 1857, *C. anceps*, Muh.

C. laxiflora, Lam., 1789, not of Schk.
Boott, Illus. Car., No. 87, Tab. 89, 1858.
Gray, Man. Bot., p. 524, 1857.
C. heterosperma, Wahl., No. 67, 1803.
C. striatula, Mx., vol. ii, p. 173, 1803.
C. anceps, Muh., Letter to Willd.
Willd., vol. iv, p. 278, 1805.
Schk., Tab. Fff., fig. 128, 1812.
C. plantaginea, in part, Schk., Tab. Kkkk, fig. 195, 1812, not Lam.
Muh. Gram., p. 242, 1817, not Lam.
C. anceps, Muh. Dewey, Sill. Journ., vol. x, p. 36, 1826.
Tor. Mon. Cyp., p. 414, 1836, and Am. Auth., var. *patulifolia*, Dew. Carey Ed. 1, and Man. Bot., 1857, and var. *plantaginea*, Boott, both Schk., fig. 195.

This case shows us the principal cause of the numerous synonyms in Caricography, viz., their being named by different botanists in different places and unknown to each other. In so large a genus, embracing more than eight hundred species, in all quarters of the globe, this multiplication of names may easily occur. This species was named by authors in different parts of the world, who knew not those already given. Hence, all the names were correct and legitimate, except the reference of this species in Muh. Gram. to *C. plantaginea*. Interesting as is the correction by Dr. Boott, the point attained shows only the author of the original or first name.

Again; where the so-called variety requires as long, or nearly as full a description, as the species itself, there is no objection to giving it the rank of a species, till it is proved that both forms are produced from the same seed or the one changes into the other in growing. In the well known varieties, as some have called them, this proof has not been attained in one case of a hundred. Some have been called varieties and so described for years, when they have been raised to the place of species, and continue to hold their rank. Besides the instance of *C. gynandra*, already noticed, there are others equally obvious and certain.

For these reasons, it is difficult for me to adopt two of the varieties of this species in Dr. Boott's splendid "Illustrations," viz., *C. styloflexa*, Buck., and *C. blanda*, Dew. Indeed, it needs but little extension of the specific description to comprehend two or three other and admitted species.

ART. XIII.—*On the Variable Illuminating Power of Coal Gas;* by WILLIAM E. A. AIKIN, Prof. Chem., &c., University of Maryland.*

(Read before the American Association for the Advancement of Science, at the Baltimore Meeting, May, 1858.)

IN common with a large number of our citizens, my attention was directed some short time since, to a somewhat sudden, inexplicable and enormous increase in the amount of our quarterly bills for gas consumed; an increase equal at times to an advance of a hundred per cent over the corresponding quarter of the preceding year. As it would have been absurd to suppose a simultaneous derangement of all the meters over an extensive district, it was obvious that the difficulty could not lie in any error in the registry of the gas, but in its illuminating power, necessarily requiring the consumption of a greater bulk of gas to produce a given quantity of light. Feeling curious to know how this difference could have occurred, I set myself to work to ascertain, if possible, what causes could be acting to diminish the illuminating power of the gas.

It has long been known that the quality of the gas produced from the fat coals is very materially influenced by the circumstances of the decomposition. In the elaborate experiments made some years ago on a most extended scale by Hedley, the British Engineer, as detailed in his report to a committee of the House of Commons, we find this subject most satisfactorily discussed. Below a cherry red heat the products obtained by heating coal in close vessels contains hardly any illuminating material. At that temperature it is furnished most freely, but after having been formed is liable to decomposition, involving a loss of carbon by contact with any highly heated surface in passing through the apparatus. Such decarbonization increasing with the degree of heat, with the extension of the red hot surface, and with the time of contact. Again, the duration of heat is most important, the best gas coming over during the first hour, the quality rapidly deteriorating, until at the expiration of four hours the product is worth very little to the consumer, and after five hours may be considered as worthless. But the bulk of such worthless gas that can still be obtained by pushing the process to completion is very considerable, equal sometimes to $\frac{2}{3}$ of all that passes over.

How far any neglect in the observance of the precautions required to produce a proper illuminating gas, may explain the result the public have no means of knowing. All that we know

* The title of the above paper was accidentally omitted when the list of papers read before the Association was published in the July No. of this Journal.

is that the manufacturers furnish an article which they say is the right article and prepared in the right way, and possessing an illuminating power varying from 14 to 17 candles. That is, their engineer reports, that on trial with a photometer, at stated times, the gas burning from a jet, consuming five cubic feet per hour, gives an amount of light equal in the average to that of 15 patent candles six to the pound. The patent candle being ostensibly a mixture of spermac ti and wax. Assuming as true all that is claimed by the manufacturers, it can still be shown that the gas even if properly made and correctly tested may be and is furnished to the consumer in a condition of greatly diminished illuminating power, compelling the consumption of a greater bulk to obtain the required light and consequently swelling the record of the meter and the sum total of the quarterly bills. In my trials to determine the specific gravity of our gas by weighing a globe previously exhausted and then filled with it, I obtained a result ranging from ·570 to ·580 somewhat below that given as characterizing good gas. But in reality I attach very little importance to this result since the mere specific gravity of such a complex mixture as coal gas can hardly be relied upon to determine its commercial value.

Although good gas certainly has a higher specific gravity than poor, yet the difference could not be taken to represent the true difference in value since the principal components of the mixture hydrogen, carbonic oxyd, light carburetted hydrogen, olefiant gas and other still heavier hydrocarbons having specific gravities, widely different, might vary somewhat in their relative proportions sufficient to affect the illuminating power, without at the same time and to the same extent affecting the specific gravity. The action of chlorine in removing the olefiant gas and other more dense hydrocarbons, the principal light giving materials of the coal gas, showed a per centage of these substances never exceeding 10 per cent. But not having time at the moment to guard against all sources of error in the process, laid it aside. My attention was principally directed to the simple inquiry to what extent will the illuminating power of the gas be impaired by keeping it in contact with water for noted periods. That it does deteriorate when thus kept, or when kept in contact with oil or even close vessels has been long known.

Dr. Ure tells us that gas from oil when first made and with a specific gravity of 1·054 will give the light of one candle when burned from jets consuming 200 cubic inches per hour. But keep the gas three weeks and then to get the same light from the same burner you must supply 600 cubic inches per hour. He adds that with coal gas the deterioration appears to be more rapid. For if such gas when first made will give the light of one candle by the consumption of 400 cubic inches per hour,

when kept four days will require the consumption of 460 cubic inches per hour to give the same light. My first attempt to obtain some definite results began on the evening of the 8th ultimo, when I filled a large receiver from the street main and placed it on the shelf of the pneumatic trough, the next evening I filled a second one and put it alongside of the first, the following evening I filled a third receiver, and still the following evening, the 11th inst., I filled a fourth receiver. On the evening of the 12th I was thus provided with four jars of gas, one of which had been standing 24 hours or one day over the pneumatic trough, this I will call No. 1; another, No. 2, had been standing two days; No. 3 had been standing three days, and No. 4 had been four days in contact with the water. The diminution in volume by such exposure was indicated by a receiver graduated to cubic inches into which I introduced 130 cubic inches of gas on the evening of the 8th; on the evening of the 12th this had lost about 10½ cubic inches, indicating a loss of about 8 per cent. of the original bulk.

The effect produced on the illuminating power of the gas by the loss of volume became at once apparent as I proceeded to contrast the value of the flames furnished by the contents of the several receivers, 1, 2, 3, and 4. I used for this purpose the ordinary photometer arrangement, taking the relative intensity of the shadows produced, as a measure of the relative intensity of the light. The candle employed for the comparison was the patent candle already referred to, and the burner was the kind known as fish tail burner, which had been previously guaged, and known to consume a trifle more than 5 cubic feet per hour with the average maximum pressure of the gas works. I need hardly add that the burner was the same in all the trials, and occupied exactly the same position. The burner and the screen on which the shadows fell were not moved at all during the experiments. The only adjustment wanted was to bring the candle nearer to or father from the screen, and by beginning with the most luminous gas the adjustment became simply a gradual withdrawal of the candle.

The capped receiver from which the gas was passed floated freely in a large glass jar, supported in an erect position by the perpendicular sides of the jar, its own weight, with all attachments, making a difference of level between the water around it and that within equal to 3½ inches, a little exceeding the ordinary evening pressure in the gas pipes. This difference of level, and consequently the pressure on the escaping gas, was kept uniform by the spontaneous sinking of the receiver as the gas was consumed, a flexible tube communicating between the stop of the receiver and the gas burner. This arrangement gave me a steady, equable flame, which continued perfectly uniform long

enough to enable me, after a few trials, to note, very exactly, its true value. The results as first obtained were too startling to be at once believed, but subsequent repeated trials satisfied me that they were very close approximations to the truth. The first trial was with the gas from the street main, which I found equal to 10·71 candles. The same gas, transferred from the pipe to the capped receiver, and burned immediately, gave exactly the same power, 10·71 candles. Gas No. 1 was next used, and found equal to only 3·50 candles; Gas No. 2, after standing two days, gave the light of 3·20 candles; Gas No. 3, three days old, was equal to 1·90 candles; and Gas No. 4, four days old, gave the light of 1·75 candles—these quantities representing the average of repeated trials.

It thus appears that the illuminating material of our coal gas is so rapidly abstracted by suffering it to remain in contact with water, that the same volume of gas which to-day will give me the light of nearly 11 candles by standing until to-morrow will give the light of only 3½ candles, and if left standing four days will give the light of only 1¾ candles, while the only means left to the consumer to get the light he requires from this deteriorated gas is to burn more of it, as we have all been doing through the past winter. If we now take into account the well known fact that gas of less illuminating power has less density, and that gas of less density passes more rapidly through a given aperture than gas of greater density, we have another cause operating to increase the consumption. In Hedley's experiments the Argand burner which gave the light of 25 candles when supplied with 3 cubic feet per hour of gas from Welsh cannel coal, with a specific gravity of ·737, required no less than 7½ cubic feet per hour to give the same light, from the same burner, when the gas was made from the Newcastle coal and had a specific gravity of only ·475.

Again, as we diminish the illuminating power of the gas we increase its heating power, and this necessarily brings with it a higher temperature given to the burners, a higher temperature given to the gas passing through them, and again an increased rapidity in the flow. It is thus manifest that the public are placed in a peculiarly unfortunate position, since all the mistakes that are likely to occur in the process of manufacture are mistakes that must inevitably increase the bills of the consumer and the profits of the manufacturer. If the workman fails to raise the heat with proper rapidity, if he overlooks a retort and allows the heat to continue a little too long, if towards the close he allows the heat to rise a little too high, the result is inevitable the product is deficient in illuminating power. Or if on any one day a little more gas is produced than is legitimately required, the surplus remains in the gasometer to vitiate the supply of to-

morrow. To what extent this vitiating action operates may be inferred from the fact that I have never been able to obtain from the gas of our pipes an illuminating power equal to the minimum of that reported by the engineer of the gas company. In my trials the power has varied from that of 13 candles down as low as that of 9 candles, instead of ranging from 14 to 17 candles.

This difference is perfectly intelligible if we assume the last quantities to represent the value of the gas when first made, and my results to represent its value as delivered to the consumer.

In conclusion I would merely add that the difficuly suggests its own remedy. And that would be to have a standard of quality established by the proper authorities, taking the illuminating power as the basis of the calculation, and then to have the requirements of such standard insured by a nightly examination, if necessary on the part of some one entirely disconnected with the manufacture. In other words the photometer can be made as available and as valuable to the consumer of gas as the hydrometer is to the spirit merchant; as he distinguishes with his instrument in any mixture, between the spirit he wishes to buy and the water he is unwilling to pay for, so the consumer of gas can distinguish with the photometer between the true illuminating material and the worthless heat producing gases, hydrogen and light curburetted hydrogen, that make up the bulk of the ordinary coal gas.

ART. XIV.—*On the Dynamical Condition of the Head of a Comet;* by Professor W. A. NORTON.

IT is proposed, in the present article, to give the mathematical theory of the development of the nebulous envelope of the head, and the tail of a comet from the nucleus; under the combined action of a repulsive force exerted by the nucleus, and a repulsive force exerted by the sun—each of these forces being supposed to vary inversely as the square of the distance from the centre of the repelling mass. So far as I am aware, no attempt has hitherto been made to develope the idea of a dynamical condition of the head of a comet into a mathematical theory, based upon precise numerical laws.

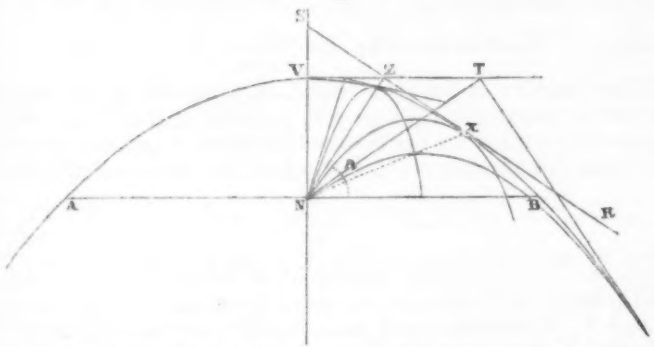
The hypothesis that a projectile force is in operation, combined with a repulsive action, or even with a gravitating force only, will also be briefly considered.

Let us first suppose cometic matter to be expelled from all points of the surface of the nucleus, on the side toward the sun, and in directions normal to the surface, regarded as spherical. As the nucleus is very small, in comparison with the nebulosity

of the head, the error will be slight if we regard it as a mere point, and conceive the nebulous matter to be repelled in all directions from this point. At the same time it must be observed that, for each expelled particle, the central point of repulsion is below the point of emission a distance equal to the radius of the nucleus. Again, as the rectilinear dimensions of the head of a comet are small as compared with its distance from the sun, we may, without material error for our present purpose, regard the repulsive force of the sun as constant.

Let N (fig. 1) be the nucleus, regarded as a point, NS the direction of the sun, and AB a line perpendicular to NS, which

1.



we will regard as the line of demarcation between the head and tail. Suppose a particle to be emitted in the direction NZ, and let the angle of projection, $ZNB = a$. Also let $p =$ acceleration due to excess of repulsion of nucleus over its attraction, at the surface of the nucleus; $k =$ opposing acceleration, from sun's repulsive force; and $r =$ radius of nucleus, or the distance of the point of emission from the centre of repulsion, wherever this may be. We will first undertake an approximate investigation, by disregarding the effect of the recess of the particle from the line NZ upon the repulsive force of the nucleus. This amounts to supposing that the centre of repulsion moves along a line perpendicular to NZ, at the same rate that the particle recedes from NZ.

Decompose the sun's repulsive force into two components,—the one along NZ, and the other perpendicular to it. The former will be $k \sin a$, and the latter $k \cos a$. Denoting by z the distance passed over by the receding particle, in the direction NZ, in any interval of time; and by v the velocity at that distance, we have,

$$\left(\frac{pr^2}{z^2} - k \sin a\right) dz = v dv.$$

Integrating, $$-\frac{pr^2}{z} - k\sin a \,.\, z + C = \frac{v^2}{2}.$$

If we suppose the initial velocity to be zero, $v = 0$, when $z = r$, and

$$-\frac{pr^2}{r} - k\sin a \,.\, r + C = 0, \text{ or, } C = \frac{pr^2}{r} + k\sin a.$$

Whence, $$pr^2\left(\frac{1}{r} - \frac{1}{z}\right) - k\sin a\,(z-r) = \frac{v^2}{2}; \text{ or, } pr^2\left(\frac{z-r}{rz}\right) - k\sin a$$

$$(z-r) = \frac{v^2}{2} \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

Let Z = greatest distance passed over in the direction NZ, and we have $$\frac{pr}{Z} - k\sin a = 0; \text{ or, } Z = \frac{pr}{k\sin a} \quad . \quad . \quad (2)$$

This value of z is the distance NZ, X being the point where the orbit is tangent to the line ZX, perpendicular to NZ. Putting $a = 90°$, we get for the distance to which a particle will recede from the nucleus, when emitted in the direction NS,

$$H = \frac{pr}{k} \quad . \quad . \quad . \quad . \quad . \quad (3)$$

But, by equ. (2), $Z\sin a = \frac{pr}{k} = H$; also $Z\sin a = Z\sin NZV = NV$; hence NV = H, and the point Z will fall on VT, drawn through V, at the distance H from N, and perpendicular to NS.

To find X, the point of tangency, resume equ. (1); and, since remote from the nucleus r may be neglected in comparison with z, we have $$\frac{dz^2}{dt^2} = v^2 = 2(pr - k\sin a \,.\, z) \quad . \quad . \quad (4)$$

Whence, $$dt = \frac{1}{\sqrt{2}} \cdot \frac{dz}{\sqrt{pr - k\sin a \,.\, z}} = \frac{1}{\sqrt{2pr}} \cdot \frac{dz}{\sqrt{1 - \frac{k\sin a}{pr} z}}$$

Integrating, $$t = \frac{1}{\sqrt{2pr}} \cdot \frac{\left(1 - \frac{k\sin a \,.\, z}{pr}\right)^{\frac{1}{2}}}{-\frac{1}{2}\frac{k\sin a}{pr}} + C$$

Or, reducing, $$t = -\frac{\sqrt{2pr - 2k\sin a \,.\, z}}{k\sin a} + C \quad . \quad . \quad (a)$$

This formula is quite accurate for determining any portion of the interval of time sought, for the beginning of which z is large in comparison with r, and as the motion is far more rapid in the vicinity of the nucleus than at a distance from it, we may obtain pretty nearly the whole interval of time, from N to X, by supposing z, at the beginning, to be several times r; but, on this

supposition the formula gives nearly the same value, as when z is made equal to zero. Therefore let $t=0$, when $z=0$, and we have $C=\frac{\sqrt{2pr}}{k\sin a}$; and

$$t=\frac{\sqrt{2pr}}{k\sin a}\left(1-\sqrt{1-\frac{k\sin a.z}{pr}}\right) \quad . \quad . \quad (5)$$

For the point X, $z=\frac{pr}{k\sin a}$, and $T=\frac{\sqrt{2pr}}{k\sin a}$ . . (6)

To verify this value of T, I have made another calculation, by dividing the time into two intervals. The first extends to the instant of time when the distance becomes equal to the $\sqrt{\frac{k}{p}}$ part of the whole distance, Z; during which the motion may be regarded as very nearly uniform, with an average velocity equal to $\sqrt{pr}$. The calculation for the remaining interval was made by the above equation (a). The result obtained is $T=\frac{\sqrt{2pr}}{k\sin a}\left(1+\frac{1}{5}\sqrt{\frac{k}{p}}\right)$. This differs from the above determination, by only $\frac{1}{85}$, in the case of the comet of 1811, and about $\frac{1}{20}$ in the instance of Donati's Comet.

To find ZX=X, we have $X=\frac{1}{2}k\cos a.T^2=\frac{1}{2}k\cos a\,\frac{2pr}{k^2\sin^2 a}=\frac{pr\cos a}{k\sin^2 a}=\frac{pr}{k\sin a}.\cot a$ . . (7)

But $ZX=ZN\times\cot ZXN=\frac{pr}{k\sin a}.\cot ZXN$; hence $ZXN=a=ZNB=NSZ$. (Fig. 1.) Thus $NX=NS$; and the point of tangency, X, of the path of any particle to the line ZR, lies in a parabola, which has N for its focus, and V for its vertex.

As the orbits of all the particles are tangent to this parabola, the paraboloid generated by revolving it about its axis will be, approximately, the bounding surface of the head of the comet.

To ascertain the form of the orbit traced, on the present supposition, by any particle, resume the value of t, as given by equ. (5); also take the value of t given by equation $x=\frac{1}{2}k\cos a.t^2$. We thus get

$$\frac{\sqrt{2pr}}{k\sin a}\left(1-\sqrt{1-\frac{k\sin a}{pr}z}\right)=\sqrt{\frac{2x}{k\cos a}}$$

Squaring, $\frac{2pr}{k^2\sin^2 a}\left(1-\sqrt{1-\frac{k\sin a}{pr}z}\right)=\frac{2x}{k\cos a}$ (approximately)

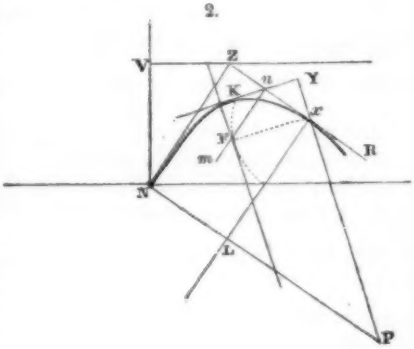
Reducing, $\left(1-\frac{k\sin{}^2a}{pr\cos a}x\right)^2=1-\frac{k\sin a}{pr}z$

Transposing coordinates to point X, we have, $x=\frac{pr\cos a}{k\sin{}^2a}-x'$, and $z=\frac{pr}{k\sin a}-z'$; substituting and reducing,

$$\frac{k\sin a}{pr}\text{tang.}^2a.x'^2=z', \text{ or, } x'^2=\frac{pr}{k\sin a}\cot{}^2a.z' \quad . \quad . \quad (8)$$

This is the equation of a parabola; the diameter through X makes an angle β with XR (fig. 2) determined by the equation $\cot\beta=\frac{2X}{Z}$; or by laying off LP=2X, and joining P with X. The parameter of this diameter=X.cot a sin β.

2.



To find the focus and vertex lay off on the diameter produced the distance $XY=\frac{\text{parameter}}{4}.\cos{}^2\beta$, draw YK perpendicular to the diameter; the vertex will be on this line. Through the point n where this line crosses the tangent XZ, draw nm perpendicular to XZ, and with a radius $=\frac{\text{parameter}}{4}$ describe a circle around X as a centre; the point F in which this circle cuts nm is the focus. Drawing FK parallel to XP, we have the axis and the vertex. The parameter of the axis $=X\cot a\sin\beta.\sin{}^2\beta=X\cot a\sin{}^3\beta=Z\cot{}^2a\sin{}^3\beta$ (by equs. (2) and (7)).

In this investigation it is virtually supposed that the nucleus moves along the line NL, perpendicular to NZ, at the same rate that the particle recedes from NZ. This may seem to be a rough approximation except in the case of a large angle of emission; but, with a certain restriction soon to be stated, the results obtained are in fact a close approximation to the truth. The reason is that the repulsive force of the nucleus produces the greater part of its effect while the particle is in its vicinity, and at a distance from it, where the change of direction and the difference between NZ and NX has become considerable, it is exceedingly small in comparison with the sun's action. To test the accuracy of our results, let us investigate the distance that

the particle will recede in the direction NZ, upon a different and extreme hypothesis; viz., that it is projected from a point at a distance $2r$ from the centre of the nucleus, with a velocity equal to that acquired in passing from the distance r to the distance $2r$, and that the repulsion of the nucleus acts in the direction NX, which, as we have seen, is inclined to NZ under an angle equal to the complement of a. The differential equation for finding the velocity in the direction NZ, will be

$$\left(\frac{pr^2}{z^2}\sin a - k\sin a\right)dz = v\,dv.$$

Integrating, $$-\frac{pr^2}{z}\sin a - k\sin a\,.\,z + C = \frac{v^2}{2}.$$

Putting V = velocity at distance $2r$,

$$C = \frac{V^2}{2} + \frac{pr^2}{2r}\sin a + k\sin a\,.\,2r$$

Whence, $$\frac{v^2}{2} = k\sin a\,(2r-z) + pr^2\sin a\left(\frac{1}{2r}-\frac{1}{z}\right) + \frac{V^2}{2} \quad . \quad . \quad (9)$$

To find V^2, we have $v'\,dv' = \frac{pr^2}{z^2}\,dz$, or $\frac{v'^2}{2} = -\frac{pr^2}{z} + C$.

$C = \frac{pr^2}{r}$; thus $\frac{v'^2}{2} = pr^2\left(\frac{1}{r}-\frac{1}{z}\right)$. Which gives $\frac{V^2}{2} = \frac{pr}{2}$, $V^2 = pr$, and $V = \sqrt{pr}$.

Substituting in equ. 9, we have

$$\frac{v^2}{2} = k\sin a\,(2r-z) - \frac{pr\,(2r-z)}{2z}\sin a + \frac{pr}{2}.$$

Putting second member $=0$, we obtain

$$Z = \frac{pr}{k\sin a} - \frac{pr}{k\sin a}\cdot\frac{1}{2}(1-\sin a)\ \text{(nearly)} \quad . \quad . \quad . \quad (10.)$$

The second term is small in comparison with the first, except for the smaller angles of emission. Taking $a = 45°$,

$$Z = \frac{pr}{k\sin a} - 0{\cdot}14\ \frac{pr}{k\sin a}.$$

We accordingly have, in general, $Z = \frac{pr}{k\sin a}$ (nearly). This is the same result that was obtained by the first investigation. That it can differ but little from the truth may be seen by comparing $V = \sqrt{pr}$, with the greatest possible velocity that the repulsive energy of the nucleus could impart to a particle, if unopposed by any force, and acting through an infinitely great

distance. The expression for this maximum velocity is $\sqrt{2pr}$; greater than V only in the proportion of 7 to 5. At the point V (fig. 1) the repulsive force of the nucleus is less than that of the sun, in the same proportion that NV is less than r, the radius of the nucleus. In the case of the great comet of 1811 this ratio was nearly as 300 to 1. Unless a comet comes very near the sun, the repulsion of the nucleus, at the outer surface of the head, is therefore so small in comparison with the sun's repulsive energy, as scarcely to have any sensible effect.

We may therefore suppose that when any receding particle reaches the tangent point X, (fig. 2), it continues on its course, subject to the sun's action alone. The preceding investigation would not, in general, apply with sufficient accuracy to the orbit continued beyond this point. The trajectory that we readily obtain on the supposition just mentioned, becomes, however, materially modified when a returning particle passes in the vicinity of the nucleus. The repulsion of the nucleus will in this case deflect the particle into a curve convex to the nucleus. When the distance is too great for this effect to be produced, the repulsion will still have the effect to diminish, more or less, the curvature of the orbit.

The trajectory at the remoter distances from the nucleus may also be obtained, by the ordinary theory of projectiles, if we regard the particles as projected from the point where its orbit becomes parallel to AB, and with the velocity which it there has. As the angle of emission, a, becomes quite small, this conception would lead to less accurate results.

We have hitherto supposed the particle, emitted from the surface of the nucleus, to have no initial velocity. If we suppose it to be projected with any velocity V, this velocity may be regarded as having been acquired in moving from a point at a distance $\frac{r}{n}$ from the centre of the nucleus; and proceeding with the investigation as before, we obtain in place of equ. (2)

$$Z = n \frac{pr}{k \sin a} \quad . \quad . \quad . \quad (11)$$

From which it appears that the effect will be to increase the value of Z, in the ratio of n to 1; without, therefore altering the form of the head of the comet. The present supposition is equivalent to conceiving the nucleus to be concentrated into a sphere whose radius is $\frac{r}{n}$, and that the particle leaves its surface without initial velocity.

Let us now suppose, for the moment, that the particle is projected from the nucleus, and is subject only to the *attraction* of

the nucleus, and the repulsion of the sun. Let g = acceleration due attraction of nucleus, at its surface; or, what amounts essentially to the same, the excess of the attractive over the repulsive acceleration. We now obtain

$$Z = \frac{V^2}{2k \sin a} - \frac{gr}{k \sin a} \quad . \quad . \quad . \quad . \quad . \quad (12)$$

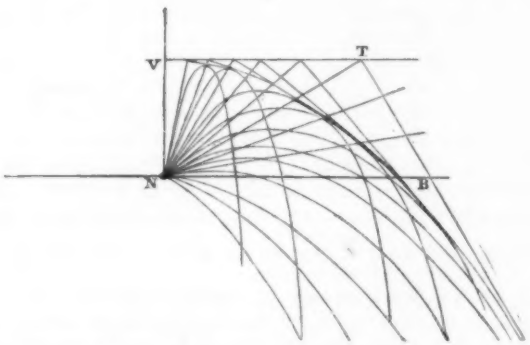
If we suppose $g = 0$, we have

$$Z = \frac{V^2}{2k \sin a} \quad . \quad . \quad . \quad . \quad . \quad (13)$$

which is of the same form as equ. (2).

In view of what has now been established we may conclude that, if the force of repulsion is of the same intensity, on all sides of the nucleus, the outer surface of the head of a comet differs but little from a paraboloid of revolution, having its focus at the nucleus and its vertex at the point V, (fig. 1) to which the particles are expelled in direct opposition to the sun's repulsive force. We see also that the paths of the individual particles are very nearly parabolas, having the positions and dimensions indicated on p. 90. Or at least this is sensibly true, from the nucleus to the tangent point X. Fig. 3 shows a number of these orbits, corresponding to various angles of emission.

3.



Such, on the present theory, is the actual limiting surface of the head. The form and extent of the surface, *as visible to the observer on the earth*, must differ more or less from this; for in the first place, at the actual boundary the line of sight touches the surface, and it is not until it falls within it and traverses a larger extent of luminous matter, that a sensible impression is made upon the eye. Again, the apparent form depends upon the obliquity to the axis under which the head is viewed..

Donati's comet, about Oct. 10th, was seen under favorable circumstances for observing the actual form of the head. The line of sight was then inclined under a large angle to the axis of the head and tail. The visible form of the head must also depend upon the various degrees of condensation, or closeness of proximity of the moving particles; and this is dependent upon the relative velocity of the particles following on after each other, and the proximity and intersections of the orbits of different particles. To illustrate, those particles which are ejected more in a direction toward the sun, have very small velocities at the turning points of their orbits; the particles are therefore crowded together there, and the visible surface should differ but little from that of the paraboloid, before mentioned. On the other hand the particles which are emitted from the sides of the nucleus, or under small angles, (a), are constantly moving with increasing velocity, and hence, those which follow each other, in the same path, must separate from one another as they recede. It is therefore only by the proximity and intersection of such separate orbits that the luminosity can be greater at some parts than at others. The tendency of this state of things will be to contract the lateral parts of the head; or in the direction NB, (fig. 3). The apparent surface of the head should therefore lie within the actual parabolic surface, and have the approximate form of an *ellipsoid*,—the form which Donati's comet presented.

A still greater deviation from the parabolic form would result if the energy of the repulsion should be less at the sides than at the anterior portion of the nucleus; or if, while the repulsion remained the same, the velocity of projection should decrease from the front of the nucleus where the sun is vertical.

If we suppose the repulsive acceleration of an emitted particle, to depend upon the action of the sun's rays upon the surface of the nucleus, and to be proportional to its intensity, then the distance to which it will be repelled, in the direction of emission, will be expressed by $\frac{pr}{k \sin a} \times \sin a$, or $\frac{pr}{k}$. On this hypothesis the form of the head would be nearly *circular*. The circular form has in fact often been observed (e. g. the semi-circular disc, or hood, resting upon the nucleus of Donati's comet, after its perihelion passage; also head of Halley's comet, on Oct. 29th, 1835).*

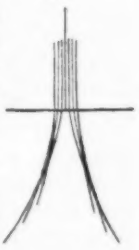
If we adopt the idea that the nucleus exercises no repulsive action; or that its repulsion, if any, is exceeded by its attraction, equ. (12), shows that the form of the head must still be parabolic.

* According to the subsequent theory of the sun's electric action the velocity of emission should, in general, be least at the sides of the nucleus. I conceive also that the repulsion should be least effective, as compared with the attraction, at the sides.

For the first term gives the parabolic form, and the subtractive term varies as $\frac{1}{\sin a}$, which is also the law of variation of the distance between two parabolas having a common focus and axis, estimated in a direction perpendicular to the tangent. The only essential difference between this case and that in which the repulsion is in excess, is that the returning particles which pass in the vicinity of the nucleus will now be attracted by it, and pursue paths more concave toward the nucleus, instead of convex.

A detailed discussion can therefore alone settle definitively the question between these two possible modes of development of the head of a comet from its nucleus. This must be left for another occasion. We will only call attention here to a remarkable luminous appearance that has occasionally been observed behind the nucleus of a comet (e. g. Comet of 1807), which seems to be a striking indication of the operation of a force of repulsion. It consists, apparently, of two jets of luminous matter, issuing from the nucleus, and convex toward each other, like the two branches of a hyperbola. Upon the theory of repulsion, it may be explained by the intersection of the orbits of particles that pass in the immediate vicinity of the nucleus. Those which make the closest approach to the nucleus, will be most deflected from their course. The consequence will be that the trajectory of each particle will intersect those of all the particles that come between it and the nucleus. This curious result is illustrated in fig. 4. It is a case similar to that of caustic curves in optics.

4.

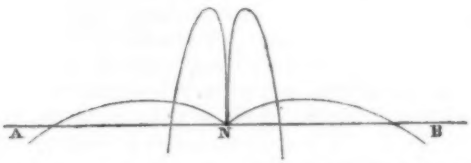


It will be seen on referring to fig. 3, that the particles which are emitted from the portion of the nucleus that lies nearest to the sun, on returning past it go to make up the body of the tail; while those which proceed from the lateral parts, as they are driven away by the sun, finally form the surface of the tail. We would here take occasion to observe that as the angle of emission becomes less, the orbits, as they traverse the head, and are about leaving it, lie nearer together; and rather suddenly separate from each other, when the angle becomes small. This fact is imperfectly indicated in fig. 3. It may be inferred from the fact that the parameter of the parabolic path of the particle continues to increase as the angle of projection diminishes, until it becomes quite small. The peculiarity here noticed affords an explanation of the sudden curved terminations of the luminous disc that formed the central portion of the head of Donati's Comet, as seen in telescopes in October; and of other similar luminous appearances (e. g. Halley's Comet as seen by Struve, Oct. 29th, 1835, and the same on Oct. 22). The natural result,

upon the normal condition of things, would seem to be modified and made more conspicuous, in some instances, by a sudden discontinuance, at a certain point on the side of the nucleus, of the ejection of luminous matter.

I have indicated on a previous occasion (in a paper read before the American Philosophical Society, in 1843) the cause of the curvature of the tail, and of the deviation of its general direction from direct opposition to the sun; on the theory of the particles being separately driven off by the repulsive force of the sun. The theory of the development of the tail is so intimately associated with that of the head—the two appearances being in fact but two different parts of one general phenomenon,—that it will be proper here to notice a particular phenomenon which appears to find its explanation in the views now presented. I allude to the fact so conspicuous in Donati's Comet, and before recognized as a general fact, that the preceding side of the tail was brighter and more distinctly defined than the following side. The phenomenon appears to result from the unequal velocities of different particles as they pass from the head into the tail. This explanation is illustrated in fig. 5. The particles which

5.



recede to a greater distance from the base, AB, of the head, pass it on their descent with much greater perpendicular velocity than those emitted under small angles from the nucleus, and attaining only to a small height above AB. The more rapidly moving particles go to the interior of the tail, the others to the marginal parts. Now the former will pursue a straighter course, as they move away from the head, and one which deviates less from direct opposition to the sun. They will, therefore, cross in succession, or crowd upon the lines pursued by the outer particles on the preceding side of the tail, and separate from those traced by the particles on the other side of the tail. This effect should be more marked as the comet recedes from the sun, and the initial velocity acquired by the central particles, becomes a larger fraction of the velocity imparted to them by the sun. It was finely observed, in the instance of Donati's Comet, about Oct. 10 and 11. As the comet receded from the earth and sun, became less distinct, and our line of sight became oblique to the plane of the orbit, the contrast was less conspicuous, and the wide dispersion of nebulous matter toward the end of the

following side of the tail was no longer noticed. Another cause is in operation, while a comet is receding from the sun, to produce the same results; the central particles, which we have seen have the greater velocity, were emitted from the nucleus before the others which leave the base of the head at the same instant, and therefore arrive with a higher velocity in the direction of the orbital motion of the nucleus. To these may be added another possible cause, in occasional operation; viz: the more copious evolution of luminous matter on the forward side of the nucleus, which, if the comet does not turn upon an axis, has been longest exposed to the sun's action.

In striking confirmation of this explanation we have the fact noticed in the case of Donati's Comet, and of comets generally, that the fainter secondary tails make their appearance at the anterior side of the tail.

Explanation of the observed variations in the size of the Nebulous Envelope of the Head of a Comet, as the Comet approaches and recedes from the Sun.—Many of the variations noticed as having taken place in the length and distinctness of the *tail* of a comet, are merely apparent. For example, when a comet is at a great distance without the earth's orbit its tail is foreshortened; but, for this reason is more distinct than it would otherwise be. The tail of the great comet of 1843 was also very much foreshortened on Feb. 28th, when it was seen in open day; to which supposed fact was attributed, in part, the great brilliancy of the comet before its orbit had been determined. But the variations in the dimensions of the head, to which we here allude are real, and general, if not universal. The facts noticed are that the nebulosity of the head contracts greatly as the comet approaches the sun, and enlarges, in a corresponding degree, as it recedes from the sun. These curious and puzzling facts are but simple consequences of the dynamical theory that has now been developed. On referring to equ. (3) it will be seen that the repulsive force of the nucleus exceeds that of the sun, at the distance of the nucleus, in the ratio of the focal distance NV (fig. 1) to the radius of the nucleus. Unless, therefore the repulsion of the nucleus varies with its distance from the sun, the ratio of these forces must vary inversely as the square of the distance from the sun. The focal distance and all the distances Z, given by equ. (2), should therefore both decrease and increase, according to the same law, as the comet approaches the sun, and recedes from him again. The contractions, observed by Hevelius to take place in Encke's comet, in 1828, as it approached the sun, were greater than would result from this cause alone. The enlargement of the envelope of Donati's comet was also, according to the measurements of G. P. Bond, Esq., of Harvard College Observatory, more rapid than should have resulted simply from

the decrease in the sun's force. We must therefore conclude, from our present stand-point that the matter of the nebulosity is either repelled or projected from the nucleus with an intensity of force that decreases as the comet approaches the sun, and increases as it recedes from the sun. Variations in the amount of luminous matter ejected may also conspire with these variations of intensity of force, to give increased effect. The changes here supposed may seem to be contrary to what would naturally be expected; I can only say in the way of explanation, that they accord with the theoretical views which I have been led to adopt on other grounds, with regard to the process of evolution of nebulous matter from the nucleus, and the nature and origin of the repulsive action.

If we suppose, in the case of Encke's, and other similar comets, that the attraction of the nucleus exceeds its repulsion, the variations in the value of k, as the comet approaches the sun, and recedes from him, will still be attended with an alternate contraction and expansion of the nebulosity. But to explain the full amount of this change, we must suppose the projectile force to vary at the same time.

Determination of the Mass and Density of the Nucleus of a Comet. —If the force of repulsion which we have here considered as exerted both by the nucleus of a comet and the sun, be a property belonging alike to all the particles of the mass of each body, we have the means of readily determining the mass and average density of the nucleus. For equ. (3) makes known the ratio of p to k, and by the law of inverse squares, we may compute the ratio of k to the repulsive acceleration that has place at the sun's surface, which we will denote by K. We then have, to find the ratio of the masses of the sun and nucleus, the proportion

$$p : \mathrm{K} :: \frac{m}{r^2} : \frac{\mathrm{M}}{\mathrm{R}^2}$$

$$\text{Which gives } \mathrm{M} = \frac{\mathrm{K}}{p} \cdot \frac{\mathrm{R}^2}{r^2} \cdot m \quad . \quad . \quad . \quad . \quad . \quad . \quad (14)$$

I have made the computations for the comets named in the following table, which contains, also, the data used in the calculations.

Name and Date.	Diameter of Nucleus.	Distance N V (Fig. 1).	Distance from Sun.	Observer.
Donati's Comet; Oct. 5th,	400 miles	4410 miles	57,000,000 miles	Bond.
" " Oct. 6th,	800 "	5120 "	57,500,000 "	"
" " Oct. 8th,	1120 "	7720 "	58,500,000 "	"
First Comet of 1811; Oct. 6th,	428 "	63,500 "	106,000,000 "	Herschel.
Halley's Comet, 1835; Oct. 29th,	465 "	25,520 "	64,000,000 "	Struve.
Comet of 1799,	373 "	20,000 "	100,000,000 "	
Comet of 1807,	538 "	30,000 "	95,000,000 "	Herschel.

In the instances of the Comets of 1799 and 1807 the date of the measurements is not known, and accordingly the distance from the sun has only been roughly estimated. The distances given for the other comets are sufficiently close approximations for our present purpose. The true diameter of the nucleus of Donati's Comet cannot exceed 400 miles, as determined on Oct. 5th; but as it is possible that the cometic matter may be ejected from the surface of the apparent nucleus, the calculations have been made from the observed diameters. The results obtained by taking the diameter of nucleus at 400 miles are given at the bottom of the table.

The following table contains the results of the calculations:

Name.	Mass Compared with Sun.	Mass Compared with Earth.	Density (density of water=1.)
Donati's; Oct. 5th,	$\frac{1}{3,691,685,000}$	$\frac{1}{10,400}$	3·8
" Oct. 6th,	$\frac{1}{1,589,306,000}$	$\frac{1}{4,476}$	1·1
" Oct. 8th,	$\frac{1}{778,578,000}$	$\frac{1}{2,193}$	0·75
First Comet of 1811,	$\frac{1}{627,654,000}$	$\frac{1}{2,332}$	13·6
Halley's Comet,	$\frac{1}{690,493,000}$	$\frac{1}{1,945}$	12·6
Comet of 1799,	$\frac{1}{2,671,219,000}$	$\frac{1}{7,524}$	6·3
Comet of 1807,	$\frac{1}{1,136,885,000}$	$\frac{1}{3,202}$	5·0
Donati's; Oct. 6th,	$\frac{1}{3,179,612,000}$	$\frac{1}{8,956}$	4·4
" Oct. 8th,	$\frac{1}{2,180,018,400}$	$\frac{1}{6,154}$	5·9

The densities, although differing considerably among themselves, and possibly, as we shall soon see, too large, indicate that the nuclei of the larger comets are not made up entirely of vapor, or gas. The most probable inference to be drawn from them, is that the nucleus of a bright comet is a body of solid matter, like the earth, more or less covered with water, of which the greater portion is ordinarily in the condition of ice.

If the masses, as above determined, are too large to be admissible, we must then draw the theoretical inference that the matter expelled from the nucleus derives part of its velocity from a *force of projection*. That such a force of projection is in fact sometimes in operation, if the present theory be true, may be seen from the results given in the following table. The calculations are here made for the outer envelope; for which the values of NV were 321,500 miles and 20,000 miles.

Name and Date.	Mass compar'd with Sun.	Mass compar'd with Earth.	Density. (density of water=1)
First Comet, 1811, Oct. 6th,	$\frac{1}{163,595,000}$	$\frac{1}{461}$	68·5
Donati's, 1858, Oct. 2d,	$\frac{1}{758,215,000}$	$\frac{1}{2,136}$	18·7

A similar indication is afforded by the apparent increase in the mass of Donati's comet, from Oct. 5th to Oct. 6th, and 8th, as given in the former table.

When the nucleus appears to be surrounded by a spherical envelope, which is not continued into the tail, as in the case of the great comet of 1811, in the formation of this inner envelope the repulsion of the nucleus may possibly be inoperative, The mass of the nucleus may be determined on this supposition, only by assuming a value for v. We now have equation (12), which, by dividing by r, and taking $a = 90°$, gives,

$$\frac{H}{r} = \frac{V^2}{2kr} - \frac{g}{k}$$

Whence,

$$\frac{g}{k} = \frac{V^2}{2kr} - \frac{H}{r} \quad . \quad . \quad . \quad . \quad . \quad (15)$$

This equation gives the ratio of g to k, for any assumed value of V, and the ratio of k to the force of gravity of the sun is made known by the velocity imparted by k to the cometic matter flowing away in the tail of a comet. The calculation may also be made for any supposed value of V, in case the sun's repulsion is inoperative.

As we do not know to what extent a projectile force is in operation, we cannot be positive that the smallest masses and densities we have determined are not too large. In fact, by hypothetically increasing the initial velocity, and diminishing the repulsive force of the nucleus, we may reduce the mass and density to any extent. We may even suppose that the nucleus neither sensibly repels nor attracts the matter projected from it; and conceive the whole effect to result from the projectile force. The velocity of projection, on this supposition would be given by the equation,

$$v = \sqrt{2k\,(H - r)} \quad . \quad . \quad . \quad . \quad (16)$$

The approximate correspondence that obtains between the densities we have computed, lends support to the idea of a general cosmical repulsion, as a property of all particles of matter, and operating under special circumstances; since it accords with the notion, probable in itself, that the nucleus is made up of liquid, or of solid and liquid matter, like the earth. If subsequent calculations made for the other bright comets should give similar results, this inference would seem almost to be established.

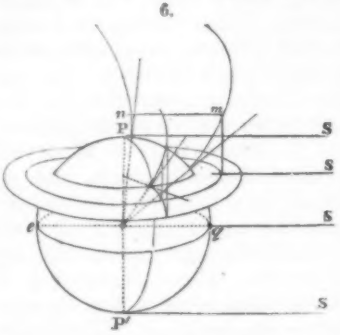
Condition of the Nucleus—Evolution of Nebulous Matter.—On these topics there is only space to give a summary of the speculative notions I have formed. In the first place I conceive the telescopic nucleus of a large comet to consist of an atmosphere of aqueous vapor, or of a vaporous and gaseous atmosphere combined, condensed upon an inner nucleus more or less covered with water, or water partly in the condition of ice. In the case of the telescopic comets this central mass is probably altogether wanting. The vaporous atmosphere of the nucleus experiences vari-

ations of electric excitement under the influence of the sun;—after the same manner that the earth's atmosphere is effected by the sun. That an electric influence is directly exercised by the sun upon the upper regions of the earth's atmosphere, or the *photosphere* of the earth, appears to me to have been established in my later papers on Magnetic Variations, published in former Nos. of this Journal. When repeated electric discharges take place in the higher and rarified regions of the atmosphere of the comet, or of that of the earth, they must have the effect, according to the results of the recent experiments of M. Plücker, to arrange the vaporous matter in columnar masses, coinciding in direction with the lines of magnetic force. We thus have *auroral* columns in the comet's as in the earth's atmosphere. At the magnetic poles of the nucleus these would have a vertical position; and from these points would gradually decline from this position, until at the equator they would lie parallel to the surface. Now as a comet recedes from the sun its temperature falls, the suspended aqueous vapor begins to condense at certain depths in its atmosphere, the electricity thus set free flows in a series of electric discharges, which follow the course of the auroral columns, as soon as they are established. Condensations extending through a considerable vertical depth in the upper atmosphere, would also be attended with electric discharges from the one elevation to the other. It is these electric discharges along these auroral columns that, as I conceive, disengage the particles of aqueous vapor, or nebulous matter so called; and impel them off with a certain velocity. The same discharges bring the expelled particles into a condition to be repelled by the nucleus. How this result may be produced cannot here be adequately explained. As the temperature of the receding comet continues to fall, the process of condensation, and consequent evolution of aqueous vaper, goes on, and the visible nucleus increases in size. It would seem, from the observations of Mr. Bond, on Donati's comet, that large masses appeared to be disengaged at certain intervals. These phenomena may have arisen from the occasional suspension of the electric discharges taking place in the upper atmosphere. This would produce the appearance of the detachment and expulsion from the surface of the nucleus, of a ring of nebulous matter. Luminous phenomena, precisely similar to those here supposed take place in the upper atmosphere of the earth, to which we have given the name of *Aurora Borealis*, and *Aurora Australis;* and probably from the same cause. They are almost uninterrupted at the pole, during the long polar winter, and only at intervals display their coruscations in the skies of the temperate latitudes; where the changes of temperature are less, and the vaporous columns assume a more oblique position. On the other hand while a comet is approach-

ing the sun, its temperature rises, and at the same time its atmospheric electricity increases; condensations of aqueous vapor and their attendant electric discharges are now much less frequent. It thus happens that the evolution of vaporous matter, to form the head and tail, is much less copious before than after the perihelion passage; and increases in quantity for a certain interval of time after it. While these *auroral phenomena*, as they may be styled, are thus subject to great fluctuations, and to sudden interruptions, and are most prevalent in the polar regions* of the nucleus, there would seem also to be an uninterrupted electric discharge, from all points of the nucleus, turned toward the sun, continually detaching particles of aqueous vapor. This should be most abundant at the regions to which the sun is vertical, and where the electric excitement produced by it is the greatest; and may give rise to the hemispherical form of envelope, (see p. 94).

The phenomenon of separate concentric envelopes, or rings, often noticed, shows that the vaporous matter set free, at any time, is not all expelled to the same distance from the nucleus. This would be the case if we were looking down upon the polar regions of a comet whose axis was perpendicular to the plane of its orbit, and the matter was detached in zones from different latitudes. The statement here made will, perhaps, be better understood on glancing at the annexed figure. It would seem also that different intensities of electrical discharge should be attended with different velocities of projection. Upon the theoretical views I have formed these electic variations should also give rise to different intensities of repulsive action, as exerted by the nucleus. Again, if all the particles set free should not be of the same size, the smaller ones would experience the greater repulsive acceleration; provided the material repulsion is of the nature of an impulsive action against the surface of the particle.

6.



If the speculative notions just presented be correct, the question arises whether the earth may not be regarded, from our present point of view, as a comet; and if so, why do we not see its luminous train. The proper answer to this inquiry would

* It is to be observed that the motion of the nucleus in its orbit, occasions a virtual rotation around an axis perpendicular to the plane of the orbit; so far as exposure to the sun is concerned.

seem to be that the earth is actually, in a certain sense, a comet, and that its luminous train is seen by us in the Zodiacal Light. The nebulous earth-ring contended for by the Rev. Mr. Jones, in explanation of his admirable observations upon the zodiacal light, would seem then, in a modified sense, to have a real existence; instead of being in a condition of statical equilibrium, as supposed, it is in a dynamical condition of perpetual dispersion and renewal.*

Nor is the expulsion of vaporous matter into the surrounding regions of space confined to the nuclei of comets, and the earth. It occurs at the surface of the sun, and perhaps of all the heavenly bodies. It is beautifully seen, as a solar phenomenon, in a total eclipse of the sun; in the *corona*, or halo that encircles the sun concealed behind the dark body of the moon; the *aigrettes* that stream out in various directions, and perhaps also the *rose colored flames* that here and there project beyond the dim circular disc of the moon.

Note.—It would seem that even the visible nucleus of a comet is not in a truly statical condition. It contracts and enlarges with the varying distance from the sun. This may be a mere appearance, arising from the varying luminosity of the photosphere. It is also possible that the inner nucleus, with its atmosphere, may be surrounded by an ethereal atmosphere, which contracts and expands by reason of variations in an impulsive action of the sun, and in the density of the ether of space, in the vicinity of the sun. These remarks may also apply to the entire envelope of Encke's comet, and the complete spherical envelopes sometimes noticed. Spherical envelopes entirely surrounding the nucleus, would also be formed if the cometic matter should be projected from all parts of the nucleus with the same velocity, but with a force insufficient to overcome the gravitating tendency. An apparent spherical continuation of an envelope behind the nucleus, might, perhaps result from the intersections of the orbits of the cometary particles urged past it into space by the repulsive force of the sun.

A more accurate determination of the orbits of the cometic particles will be given in a succeeding No. of the Journal.

Art. XV.—*Review of Hall and Whitney's Report on the Geology of Iowa.*†

The first volume of the Report on the Geology of Iowa has just been published, and in a style highly honorable to the State. The State embraces much that is of scientific and practical interest, and Professors Hall and Whitney who have had the survey in hand, have accomplished great results considering the time afforded for exploration. The Carboniferous formation is a prominent feature in the geology, and has afforded a splendid collection of

* The vaporous matter which is incessantly streaming off from the sun into remote space, should enhance the brightness of the zodiacal light.

† Report on the Geological Survey of the State of Iowa: embracing the results of investigations made during portions of the years 1855, 56 and 57. By James Hall, State Geologist, and J. D. Whitney, Chemist and Mineralogist. Published by authority of the Legislature of Iowa. Vol. I. pp. 724. 1858.

Crinoids for description and illustration. Even the Permian has some representative beds, though discovered too recently—the Introduction states, after the Permian fossils of Kansas had been made known—to be described in detail in this volume. Prof. J. D. Whitney contributes chapters on the Physical Geography, Geology, and Chemical and Economical Geology including Mines,—Prof. Hall, on the General Geology and Palæontology, and A. H. Worthen, (Assistant,) on the Geology of the Des Moines valley and some other parts of the State.

The geological investigations have thus far been confined to the eastern half of the State, the western portion of Iowa, which is equal in area to New York, Massachusetts, Connecticut and Rhode Island put together, not having yet been examined. The work is divided into eight chapters, or divisions, of which the first is devoted to the Physical Geography, the second to some general remarks on the Geology of the Northwest, the third to a review of the Geological formations occurring in Iowa, as exhibited in a section on the Mississippi river, the fourth, fifth and sixth to the detailed Geology by counties, the seventh to the Economical Geology, and the eighth to the Palæontology, the whole work being comprised in 724 pages, with 29 steel and lithographic plates of fossils, a geological map and a diagram of the leadbearing crevices near Dubuque.

In Chapter I, we have a concise view of the topography of Iowa. The State is a vast plain, gradually rising as we proceed from the east towards the west, and from the south, northwards:—the absolute elevations are given from the railroad surveys, when such could be obtained. In this plain the streams have cut deep and narrow valleys, which are bordered by precipitous "bluffs," as they are termed, the river-bottom, bluff and prairie being the three conspicuous features of the topography of the State. The height and steepness of the bluffs decrease gradually from the north towards the south. The course of the tributaries of the two great rivers forming the eastern and western boundaries of the State, the Mississippi and Missouri, is adduced as evidence of the direction of the drainage having been determined by two sets of low and narrow flexures of the strata, one set running in a northwest and southeast direction and the other nearly at right angles to this. The Wapsepinicon is mentioned as a remarkable instance illustrative of this; this river having a length of about 250 miles and draining a valley which is only from eight to twelve miles in width. The "mounds" which break the monotony of the landscape in the lead region, are described as outliers of Niagara limestone, overlying strata of the Hudson river group, and rising in isolated, flat-topped hills, presenting evidence of extensive denundation. These mounds are from 400 to 600 feet above the Mississippi river, and about 200 above the general evel of the prairie at their bases.

The prairies, those wonderful treeless and fertile plains, the most marked feature of the Northwest, are described and the subject of their origin, a much controverted question, is briefly discussed. The theories which have, at different times been brought forward to account for the absence of trees in the prairie region are discussed and pronounced to be inadequate. It is attempted in the report to show that the extreme fineness of the particles of which the soil is made up is the predominating cause of this peculiar condition of the vegetation, and some facts are stated which go to confirm this theory. Reasoning from analogy of the smaller prairies to the thickly wooded region of the Upper Peninsula of Michigan, it is inferred; "that the whole region now occupied by the prairies of the Northwest was once an immense lake, in whose basin sediment of an almost impalpable fineness gradually accumulated, under conditions, the discussion of which is postponed to another volume in which the drift phenomena of the Northwest will be taken up; that this basin was drained by the elevation of the whole region, but, at first, so slowly, that the finer particles of the superficial deposits were not washed away, but allowed to remain where they were originally deposited. After the more elevated portion of the former prairies had been laid bare, the drainage becoming concentrated in narrower channels, the current thus produced, aided perhaps by a more rapid rise of the region, acquired sufficent velocity to wear down through the finer material on the surface, wash away a portion of it altogether, and mix the rest so effectually with the underlying drift materials, or with abraded fragments of the rock in place, as to give rise to a different character of soil in the valleys from that of the elevated land. This valley soil, being much less homogeneous in its composition, and containing a larger proportion of coarse materials than that of the uplands, seems to have been adapted to the growth of forest vegetation; and in consequence of this, we find such localities covered with an abundant growth of timber."

"Wherever there has been a variation from the usual conditions of soil, on the prairie or in the river-bottom, there is a corresponding change in the character of the vegetation. Thus, on the prairies we sometimes meet with ridges of coarse material, apparently deposits of drift, on which from some local cause, there has never been an accumulation of fine sediment: in such localities we invariably find a growth of timber. This is the origin of the groves scattered over the prairies, for whose isolated position and peculiar circumstances of growth we are unable to account in any other way."

At the close of the chapter on Physical Geography, a review is given of the meteorological observations which have been made in different parts of the State by different observers. From

a comparison of these some light is thrown on another question which has been discussed without having been satisfactorily answered, namely: What is the difference between the climate of the Eastern and Western States on the same parallel? It appears that while there is but little difference in the mean temperature of the *year* on the Atlantic coast and in the Mississippi valley, in the same latitude, there is a perceptible tendency to extremes in the mean of the *seasons*, the summers being hotter and the winters colder, as we go farther west. The annexed table (from p. 33) will serve to illustrate this statement.

Locality,	Latitude.	Year.	Temperatures. Spring.	Summer.	Autumn.	Winter.
West Point, N. Y.,	41° 23′	50°·7	48·7	71·3	53·2	29·7
Fort Armstrong, Ill.,	41 30	50 ·3	50·5	74·1	51·7	24·9
Council Bluffs,	41 30	49 ·3	49·3	74·7	51·4	21·7
Utica, N. Y.,	43 06	45 ·7	44·5	66·5	47·3	24·5
Prairie du Chien,	43 05	47 ·6	48·7	72·3	48·3	21·2
Potsdam, N. Y.,	44 40	43 ·6	42·9	66·3	45·4	19·8
Fort Snelling, Min.,	44 53	44 ·6	45·6	70·6	45·9	16·1

In the geological portion of the Report, we notice the following matters as of more especial interest.

The members of the geological series developed in Eastern Iowa, all belong to the Palæozoic system, and include groups of strata from the Potsdam Sandstone up to the coal measures. The existence of the Permian in the central portion of the state is inferred from the presence of large masses of gypsum overlying the coal measures, but in connection with which no fossils have as yet been discovered. The range and extent of the formations is exhibited on the geological map accompanying the Report, a glance at which will be more satisfactory than an attempt at description. It may simply be noted that the general trend of the formation is northwest and southeast, and that the dip being to the south and west, the traveller, in passing over the State from northeast to southwest, crosses successively higher groups. There appears to have been but little disturbance of the strata since their deposition, and no igneous or metamorphic rocks are known to exist within the limits of the State.

The Silurian series, as it is developed in the Northwest, is made up of alternations of sandstones, dolomites, limestones, and shells. The order of succession, lithological character and thickness of the different members which are recognized in Iowa, may be seen in the annexed table, arranged in an ascending order:

Name of Group.	Lithological character.	Thickness.
Potsdam Sandstone,	Pure silicious sandstone,	250–300 feet.
Lower Magnesian Limestone, (Calciferous Sandstone of N. Y. Rept.)	Dolomite,	250 "
Upper, or St. Peter's Limestone,	Pure silicious sandstone,	80 "
Trenton, or Blue Limestone,	Alternation of slightly argillaceous limestones and shells, with pure limestones,	100–120 "

Name of Group.	Lithological character.	Thickness.
Galena Limestone,	Dolomite,	250–300 feet.
Hudson River Group,	Impure silicious shells and thin limestone bands,	80–100 "
Niagara,	Dolomite,	250–300 "
Le Claire Limestone, not recognized except on the Mississippi river,	Dolomite,	?
Onondaga Salt Group,	Dolomite, a few thin outliers, only recognized on the Miss. river.	

In general there is shown a thinning of the members above the calciferous sandstone as compared with the series in New York, and a disappearance of some of them. A few particulars with regard to the various groups are here added.

Potsdam Sandstone.—The very great thickness of this formation, which has been shown to exist in the Lake Superior region, is limited to the vicinity of the trappean rocks. Proceeding southwesterly from the copper-bearing range, we soon find the conglomerate to have disappeared, and have no evidence in Iowa and southern Wisconsin of the existence of more than 400 feet at any one point, while the mean development is probably not over 250 or 300 feet. The exposures of this rock are very limited in Iowa, but it covers considerable surface in Minnesota, and still more in central Wisconsin. Bands or intercalated masses of conglomerate are almost entirely wanting in the sandstone; these, as well as the lines of oblique lamination, appear to be confined to the vicinity of the igneous rocks. There is no member of the series more persistent, both in lithological and palæontological characters in the lower sandstone; it has been traced from lon. 73° to lon. 104°, exhibiting everywhere the same granular silicious character and characterized by the same organic forms, and we have, up to this time, no evidence of the existence of organic remains below this formation.

Lower Magnesian Limestone.—This is a mass of dolomite, having a thickness of from 225 to 250 feet, about 200 of which are nearly pure, crystalline dolomite, containing from one to ten per cent. of silicious sand, mechanically intermixed; the remaining 25–50 feet are beds of passage into the sandstone below, consisting of mingled and alternating sandstone and dolomite. Fossils are extremely rare in this member of the series; a few have been observed in Wisconsin in a very imperfect state of preservation, but none in Iowa.

Upper or St. Peter's Sandstone.—This repetition of the sandstone underlying the lower magnesian, is also remarkable for its persistence in lithological character and thickness over a great extent of surface. From La Salle, in Illinois, where it makes its appearance in a low axis of elevation, underlying the coal-measures unconformably, to St. Paul in Minnesota, a distance of over 400 miles, this sandstone hardly varies more than ten feet from its normal thickness of about 80 feet, which indicates a remark-

able uniformity in the physical conditions prevailing at the time of deposition of this comparatively thin mass. The fact that this sandstone is so persistent in its thickness and lithological character; that it consists of almost chemically pure quartz, in the form of grains of minute, but uniform size, with crystalline facets; that it contains no pebbles or fragments which can be recognized unmistakably as being of foreign or detrital origin, are noticed as giving plausibility to the supposition that it was a chemical precipitate, rather than the result of the mechanical disaggregation of pre-existing quartzose rocks. No fossils have been found in this sandstone.

Trenton Limestone.—Under this head is designated the series of beds between the Upper Sandstone and the Galena limestone, which may be subdivided into two portions: *a*, the buff limestone, an impure dolomite, containing from ten to twenty per cent of sand and clay; it is from fifteen to twenty feet thick, and is, in the vicinity of the Mississippi river, quite destitute of fossils. It is succeeded, in the ascending order, by *b*, the blue or Trenton limestone proper, a series of calcareous and calcareo-argillaceous layers, lime unaccompanied by magnesia appearing here for the first time in the series, the whole having a thickness of from 70 to 80 feet. For the first time, also, we find traces of organic life abundantly disseminated through the rocks, a fact not without significance in its relations to the absence of magnesia noticed above. Many of the layers, and shaly partings between the compact calcareous beds, are crowded with forms either identical with, or closely allied to, those which characterize the Trenton limestone in its extension from New York through Canada and on the northern shores of Lakes Huron and Michigan, and as far west as the Mississippi, along a line of outcrop some 1500 miles in extent.

Galena Limestone.—The passage from the Trenton limestone into the next succeeding member of the series, the Galena limestone, is not an abrupt one; on the contrary, there are, in many localities, several alternations of calcareo-magnesian and purely calcareous layers, indicating that the change of conditions which resulted in the deposition of the highly crystalline dolomite which overlies the Trenton was not effected at once, nor without occasional partial returns to the former state of things. The Galena limestone, as usually developed, is a rather thick-bedded, light greyish, or light yellowish grey, dolomite, distinctly crystalline in its texture and usually rather coarse-grained. The more crystalline portions frequently contain cavities lined with small crystals of brown spar, and the rock is remarkable for the irregularity with which it weathers, leaving picturesque outliers, with castellated forms, like watch-towers, or the half-ruined walls of ancient fortified cities. The quantity of insoluble matter in

this dolomite is very small, not usually exceeding two or three per cent; it consists almost entirely of quartzose sand. The fossils of the Galena limestone are closely allied to those of the Trenton, although, in the lead-region, certain ones, such as the *Receptaculites* and *Lingula quadrata*, which are characteristic of the Galena limestone, are not found in the underlying blue. The fossils of the Galena are all in the form of casts with the exception of those in which the shell originally consisted of *phosphate* of lime instead of the *carbonate*, as was the case with the *Lingula;* it appears, therefore, that the chemical changes which the rock has undergone since its deposition have been such as to remove the substance of shells consisting of the carbonate of lime, but to leave the phosphate untouched. Another interesting fact in connection with the palæontology of the Galena limestone is, the discovery in it of a single specimen of *Halysites catenulatus*, the characteristic and most abundant fossil of the Niagara limestone in this region. This coral was in the form of a cast, while those of the Niagara are uniformly silicified, this, with other circumstances, removing all possibility of error in regard to its true locality. A single specimen of the same genus is described by Mr. Hall as occurring in the Hudson river group on Green Bay, these two being the only instances in which this genus has been found in the United States in Lower Silurian rocks.

The Galena limestone forms a very important member of the series in the Upper Mississippi valley and in Wisconsin, although not distinctly recognized to the eastward of the Menomonee river; it is fully 250 feet thick in the vicinity of Dubuque, where it has its maximum development, and from which point it gradually thins out in every direction. It is, economically, of high interest, from the fact of its being the chief repository of the lead ore which has been, and still is, so extensively mined in the Upper Mississippi lead-region.

Hudson River Group.—This member of the series according to the Report is first distinctly recognized in its extension west of Little Bay des Noquets. Being composed chiefly of silicious and silico-argillaceous shales, which disintegrate with rapidity, so that a good natural section is rarely exposed, its existence in the Upper Mississippi valley was for a long time overlooked, although many of the shafts in the lead region are sunk through a greater or less thickness of it, in order to reach the underlying limestone. It did not escape the observing eye of Percival, who mentions it in his first annual Report, dated 1855, under the name of "Blue Shale," but without any indication of its palæontological relation or thickness, as, indeed, it has, within the limits of the lead-region in Wisconsin, been almost entirely removed by denundation. The thickness of these shales, when fully developed is from 60 to 80 feet, and in some places perhaps, as much as

100, but no natural section has been observed exposing more than 25. The quantity of organic remains crowded into some of the layers of this group, is truly astonishing; some strata of six or eight inches in thickness are made up of Orthocerata packed as closely together as they can lie. The palæontology of this formation has not been investigated in detail, but its position and the general character of its fossils leave no doubt of its equivalency with the Hudson river group.

An interesting fact in connection with these shales, is the large amount of bituminous matter which they contain, and which is shown in this Report to be characteristic of this group from New York, through Canada, to the Mississippi river. A specimen of a dark chocolate-colored shale from Savannah, Ill., was found to contain 20·96 per cent of combustible substances: other specimens from the vicinity of Dubuque, lost, on ignition, from 11 to 16 per cent. of organic matter. The black, highly-glazed and apparently very carbonaceous shales of the Hudson river valley, which have been so frequently mistaken for coal, contain from one-half to one per cent of carbon, but no volatile matter; while specimens of the Utica shale from Herkimer Co., on the other hand, lost from 12 to 14 per cent of their weight when burned in oxygen.

The presence of carbon in the shales of the Hudson river group over so extensive a region, and in so large quantity, is not only a matter of very considerable economical importance, as indicating a source from which, in those parts of the country where the true Carboniferous rocks are wanting, a supply of material for lighting, and perhaps heating, purposes may be obtained; but it is also of great interest in a theoretical point of view, as bearing on the question of the origin of the carbon in the coal-measures themselves. These shales and slates seem to have been accumulated under conditions somewhat resembling those which prevailed during the deposition of the Carboniferous series, while the presence of so large a per-centage of carbon in them is rendered still more striking by the fact, that, in the Northwest, neither the rocks below, nor those above as far up as the coal-measures, contain more than the merest trace of carbonaceous matter. From the base of the Potsdam to the top of the Galena limestone, the whole amount of carbon present in the rocks, would not, if collected into one layer, make a deposit of more than an inch or two in thickness; but if the bituminous matter of the Hudson river shales at Savannah, were all collected by itself in one stratum, instead of being diffused through perhaps 60 or 80 feet of shale, that stratum would have perhaps equalled twenty feet or more in thickness. A further investigation into the exact nature and distribution of the bituminous matter is contemplated.

Niagara Limestone.—This is the third great mass of dolomite, which, throughout the valley of the Upper Mississippi, lies next above the Hudson river shales, and which, as well from the extent of surface covered by it as from its thickness and persistency of lithological and palæontological characters, forms one of the most important members of the Silurian series in the Northwest. It is one of the rocks which, prior to the recognition of the Hudson river shales in the Northwestern mineral region, was included under the designation of "Cliff limestone;" and, more recently, has been described as the "Coralline and Pentamerus beds of the Upper Magnesian limestone,"—the term "Upper Magnesian limestone," according to Dr. Owen, including all the members of the series from the base of the Galena, up to the base of the Hamilton group. The Niagara limestone of the region in question is a nearly pure dolomite having a crystalline structure and a light yellowish gray color; it differs but little in external appearance from the Galena limestone, and hand-specimens of the two rocks might frequently be mistaken for each other. The Niagara limestone, however, does not often exhibit that tendency to irregular decomposition, and consequent weathering in fantastic forms, so characteristic of the Galena; it also contains a greater amount of silica in the form of layers and nodules of flint, and it differs from this last mentioned rock also, in the fact that the fossils it contains are usually silicified, and not preserved in the form of casts simply. It may also be noticed that, whereas in the Lower Magnesian and Galena groups the amount of magnesia present is almost exactly that required to form with the lime and carbonic acid, the double carbonate, or dolomite; in the Niagara, on the other hand, there is frequently a small excess of lime over the magnesia. The thickness of this member of the series is estimated at about 350 feet, 250 feet being the greatest amount measured in any one exposure. The Niagara limestone throughout the Northwest is marked by the presence of beds crowded with the *Pentamerus oblongus*, as also by numerous corals, of which *Halysites*, *Favosites*, *Heliolites*, *Syringopora* and *Lyellia* are the most conspicuous genera. There are also numerous Crinoids identical with, or closely allied to, those of the Niagara limestone of New York, but mostly in a very bad state of preservation, as might be expected in a crystalline dolomite.

Le Claire Limestone.—Of the groups recognized, in New York, as intermediate between the Niagara and the base of the Devonian, but a meagre representation has been observed in Iowa, and that only on the Mississippi river. The Le Claire limestone is described by Mr. Hall as a very heavy bed of dolomite, several hundred feet in thickness, which, in consequence of its disturbed condition and enduring character, has been the cause of the

Upper Rapids of the Mississippi. The fossils in it are all in the form of casts, and among them are, a small *Spirifer*, a *Spirigera*, a *Pentamerus*, undistinguishable from *P. occidentalis*, several Gasteropods and some chambered shells. This dolomitic mass is placed, conjecturally, on a parallel with the Galt limestone of Upper Canada, hitherto supposed to form the base of the Onondaga Salt Group. The Le Claire limestone has not been traced to any distance from the river, and is certainly wanting in northeastern Iowa, where the Niagara limestone is overlaid directly by rocks of the Hamilton group. It appears that a further examination should be made of the section at the Upper Rapids, as it is difficult to understand how so thick a mass of rock should appear and disappear without having been recognized anywhere except at that one point.

The Onondaga Salt Group, a member of the Silurian series of so much economical importance in New York, is represented in the Mississippi river section by a few feet only of magnesian limestone, or nearly pure dolomite, although soft and destitute of crystalline structure. The peculiar physical condition of the Onondaga Salt Group, as it exists in New York, is exhibited in some of the layers of the section on Quarry creek, a small tributary of the Mississippi, but the economically valuable minerals are wanting. This group has not been traced west of the Mississippi, where, indeed, it exists only in a few detached fragments.

Of the rocks of Devonian age, the *Upper Helderberg Limestone*, so well marked in New York and Ohio, is with difficulty to be recognized to the west of the Mississippi. Certain non-fossiliferous strata cropping out on the bank of that river, at, and for two or three miles above, Davenport, are referred by Mr. Hall to that group, chiefly, as it appears, from their stratigraphical position and lithological character.

The Hamilton group is an important member of the series in Iowa, covering many hundred square miles of surface, although greatly diminished in thickness from what it was in New York. It consists of a series of purely calcareous and calcareo-magnesian strata, with occasional bands in which argillaceous matter occurs to some extent, the lithological character of this portion of the series being somewhat more variable than that of the groups below. The greatest thickness of this group exposed in any one section appears to be about one hundred feet; but its entire development has not been satisfactorily ascertained. A few of the species of fossils found in it are identical with those occuring in New York; but most of them are new. Several are more like Devonian species of Central Europe than any which had been previously described from this continent.

The Chemung group in the Mississippi valley gives but a meagre representation of the same series of rocks in New York, Pennsylvania and Ohio. It contains almost an entirely new Fauna, although of species closely allied to those of more easterly localities of the same group.

The passage from the Devonian to the *Carboniferous series* is shown, in this Report, to be an almost imperceptible one, both in the physical and palæontological character of the groups, there being no strong line of demarcation separating the upper calcareous beds of the Chemung group from the Burlington limestone, the lowest member of the Carboniferous limestone series.

One of the most interesting facts brought out in this connection, is the existence of five distinct members of the Carboniferous limestone series. These are shown to have been deposited in an ocean which was gradually contracting its limits on the north, the greatest development of each successive member of the series, in an ascending order, being to the south of the one below it; while, subsequent to the deposition of all these and the sandstone which separates the fourth and fifth limestones, the entire area was submerged, allowing the coal-measures to be deposited on the slightly inclined edges of all these limestones, as well as of the Chemung and Hamilton rocks, and also, to some extent of the Silurian limestone, after they had been disturbed and denuded.* All these limestones of the carboniferous series are well characterized by the fossils they contain.

The fossils of the survey are described and figured in Part II on Palæontology, by Prof. Hall, this portion of the volume extending to 250 pages. The Devonian and Carboniferous series have been selected for illustration, as the Silurian had previously received much attention in the Reports of Dr. Owen. In the Hamilton and Chemung groups the fossils are particularly interesting, as exhibiting the influence of geographical conditions, or of distance, while the physical condition remained nearly the same as in central and western New York. The entire number of species described from all these rocks is about 250.

Special attention is given in the Report to the fossils of the Carboniferous limestones, as illustrating the successive members of the series; and with this object contrasting forms have not been selected, but, on the contrary, the more common and characteristic fossils of each rock. Many hitherto believed to be identical with European species are proved to be quite distinct.

The number of species of Crinoids, described in the volume, is probably equal to or greater than all those before made known

* See this Journal, [2], xxiii, 187.

from the same formations. Those of the Carboniferous limestones amount to one hundred and seventeen species, and of these *eighty-nine* are new or not before described.

The true generic characters of *Zeacrinus*, *Agaricocrinus* and *Agassizocrinus* of Troost,—genera which that author had given in his catalogue, but of which he had never published the full descriptions—are here for the first time given. In this and other cases Mr. Hall has evidently aimed to recognise fully the unpublished labors of Prof. Troost. He has on page 544 the following note:—

"I have transcribed these observations, as well as the specific description of this species, from the MS. of Dr. TROOST'S memoir upon the Crinoideæ, which is to be published in the Smithsonian Contributions to Knowledge; having been permitted to make such references and citations as would enable me to verify any of the genera and species which I might describe in the Iowa Report. By this means, although Dr. TROOST'S paper has not yet been published, he has the precedence which belongs to him."

The *Scaphiocrinus* in its typical species so nearly resembles the *Graphiocrinus* of De Koninck and Le Hon that Mr. Hall is led to suspect that they have overlooked a series of small basal plates. These characters are here illustrated; and in the *Forbesiocrinus* is shown a series of three basal plates below the five plates supposed by these authors to constitute the base. This genus is further sustained by five American species, all of which are new.

Some interesting facts are shown, for the first time, connected with the structure of the *Actinocrinus*, and particularly the distribution of the arms and their relations to the rays or radial series of the plates of the body. These relations, as well as other important points for the discrimination of species are shown in the diagrams accompanying many of the descriptions with a formula of numbers belonging respectively to the anterior, antero-lateral and postero-lateral rays. These characters, shown to be constant, offer important additional facilities for the determination of species, especially where specimens are imperfect.

Of the genus *Actinocrinus* alone twenty-nine new species and two varieties are described, and of the genus *Platycrinus* sixteen new species. *Rhodocrinus* is noticed for the first time among American Carboniferous species. Five species of the genus *Archæocidaris* are described and illustrated, one from each of the limestones of the Carboniferous series.

This Report is doubtless the best contribution yet made to our knowledge of the Crinoids and other Echinoderms of the Carboniferous system; and, both as regards their structure and their geological distribution, it is of the highest interest. We might cite at length important observations on the genera and species,

did space allow. The plates are all good, and those of Crinoids remarkably beautiful and effective.

In the chapter devoted to Economical Geology, we find a large number of analyses of the rocks, coals, and other materials of economical value occurring in Eastern Iowa. The limestones analyzed are many of them almost chemically pure dolomites; and, in general, the predominance of crystalline carbonates of lime and magnesia over the purely detrital rocks is very marked all through the series. There is a striking deficiency of the argillaceous element, especially, in all the Silurian rocks. The analyses, taken together, exhibit a tendency, as we rise in the geological scale, to a greater variety of lithological character in the members of the successive groups, a greater amount of detrital matter and a diminution in the quantity of magnesia, there being no heavy and persistent bed of dolomite above the Silurian.

Among all the specimens examined, the only ones found to contain a sufficient quantity of insoluble matter to be available for hydraulic lime are those from the Buff limestone, at the base of the Trenton. It remains to be ascertained, by practical trials, how far the dolomites and highly magnesian limestones, with but a small quantity of insoluble matter, can be used for hydraulic purposes, as they have been to a limited extent in Virginia and also in France.

The analyses of numerous samples of coal show that they belong, like all the Western coals, to the highly bituminous class; they contain from 35 to 40 per cent of bituminous matter, and from 45 to 50 of fixed carbon. They all appear to hold a large amount of hygrometric moisture with great tenacity, parting with it slowly, and not until after years of exposure to a dry atmosphere. Some samples give as much as fifteen per cent of water, expelled by drying at a temperature of 212° F. Sulphur is present in all these coals, in a form not perceptible to the eye, to the amount of from one-half of one to two per cent, and also in much larger quantity, in combination with iron and lime, as pyrites and gypsum, which substances materially impair the value of western coals. No workable iron-ore of any importance has been discovered in connection with the coal-measures of Iowa, which are exceedingly thin, no section having been measured in the Des Moines valley giving much over a hundred feet in thickness.

The subject of the occurrence of the lead ore in the Lower Silurian limestones of the Upper Mississippi valley, and, especially, within the limits of Iowa, is discussed in this Report on pages 422 to 468. The principal crevices, or lead-bearing fissures, which have been worked in the vicinity of Dubuque are described, and a diagram given illustrating their surface-

arrangement, so far as could be made out from the information collected. In no part of the lead region are the crevices developed on a more extensive scale, or with so much regularity, as in the Dubuque district. The characteristic form of occurrence of the ore is the *cave-opening*, or expansion of the vertical crevice into a cave or chamber, whose walls are sometimes lined with a heavy incrustation of pure galena, but which are more generally partially filled with clay and loose masses of rock, mixed with fragments of ore, derived from the decomposition of the material which once filled the *opening*, or metalliferous portion of the rock. Some of these caves have yielded several millions of pounds of galena, from a space very limited in depth and length.

It is a favorite idea among those who have little acquaintance with mining operations in general, or who wish to dispose of abandoned lead-mines to Eastern capitalists, that the lead crevices extend indefinitely downwards, and that the only reason why deep mining has not been carried on in this region is that the miners have not sufficient skill or capital to work down to any considerable depth. It has also been insisted on by the same class of persons, that the Lower Magnesian limestone is a good mineral-bearing rock, and that lead-mining may be carried on in it with profit, while the expediency of sinking shafts through the Upper Sandstone into this rock in search of ore, has often been discussed and urged by them. In regard to these points, of so much economical importance to the lead-region, the Report has the following:*

"There is very little evidence that the crevices continue to be productive, in the Dubuque district, even as low down as the Blue limestone; and it is certain from the study of the whole region, that they are everywhere completely cut off by the Upper sandstone. In no instance, so far as we have been able to learn, have the lodes been found to extend more than a short distance into the sandstone, or to be productive of galena in that rock. It is true that, in some localities, ore has been found in the limestone underlying this sandstone (the Lower Magnesian), when this rock occupies the surface; but the deposits in that geological position are very few in number, and the ore limited in quantity; we have yet to learn of a single instance in which diggings in that rock have been profitable for any length of time. But, again, even if the Lower Magnesian were a good mineral-bearing rock, there would be little encouragement to continue sinking from the Galena limestone, through the sandstone, into the underlying sandstone; for there is no reason to suppose that a crevice, after being entirely interrupted in the sandstone, would be resumed in the limestone below, at a point exactly in the line of direction of the workings above. A miner would be no more justified in

* Report p. 462.

sinking through the sandstone, in the expectation of meeting a continuation of his crevice in the Lower Magnesian, than he would be in commencing a shaft anywhere at random in this rock, without regard to surface-indication, and expecting to strike a valuable lode. He might possibly find one; but the chances would be more than ten-thousand to one that he would not."

That the amount of lead produced in the Upper Mississippi region is gradually diminishing is evident from the statistics; the maximum produce of these mines was in the years 1845–47, when it was nearly 25,000 tons per annum. At present it amounts to less than half that. In answer to the question, what can be done to develop the mining interest of this region, a systematic topographical and mining survey of the whole lead-bearing region is urged as an indispensable preliminary to future successful explorations. A more or less symmetrical disposition of the crevices will be found to prevail, and from the symmetry of the known, the position of the unknown may possibly be ascertained. There is no doubt that heavy bodies of ore yet remain concealed under the thick covering of drift, which makes surface explorations so expensive, and that a large amount of labor is wasted in fruitless search for workable lodes which might be more profitably expended if more systematically directed.

The existence of zinc ore in sufficient quantity and under suitable conditions with reference to fuel, labor, and a market, is pronounced highly questionable. Gold is not to be looked for, except in the most minute quantity, a caution inserted with special reference to the gold-fever raging in Central Iowa, at the time this portion of the Report was passing through the press. Specimens of considerable size may, possibly, have found their way out of the pockets of returned California miners, into the soil of Iowa.

In closing the Report, we would express our earnest hope that the survey so well begun, may be continued to its completion, and that other volumes as valuable may soon follow.

ART. XVI.—*Correspondence of Prof. Jerome Nicklès, dated Paris, October 26th*, 1858.

Scientific Association at Carlsruhe.—We ask our readers this time to go beyond the bounds of France across the Rhine. We need make no apology for this, as science is of all countries, and the announcement of scientific news is our special duty as correspondent of the American Journal.

The German Scientific Associations are generally highly interesting, as much for the men that attend them as the subjects discussed. Few meetings however have been as important as this 34th, held at Carlsruhe,

the capital of the Grand Duchy of Baden. It was remarkable for the sympathy between the government and the people, and also for the men there gathered and the papers brought forward. The first of the chemists and physicists of Germany were there; and geology, mineralogy, botany, zoology, and medicine had equally distinguished representatives. As the meeting was divided into sections, we could not hear all, and selected those departments according with our own predilections,—physics and chemistry; and we therefore confine our communications to facts brought out in these two sections.

The presidents of the section of chemistry were successively Liebig, Wöhler, Schönbein, and on the declinature of Bunsen, H. Rose. The sessions began on the 17th of September.

Schlossberger on the property of ammoniacal oxyd of copper dissolving cellulose.—This property was made known some years since by Schweitzer. Not only cellulose but also silk is soluble in this reagent. The ammoniacal sulphate of copper acts as a solvent only from the excess of oxyd of copper present. Mr. Schlossberger finds that the solvent power increases with the proportion of copper, and that the hydrate of copper dissolved in ammonia acts better than the sulphate.

The cupro-ammoniacal liquid does not dissolve gum, dextrine, starch, while it does dissolve filtering paper. The salts, and especially the alkaline salts, precipitate this solution of cellulose, and suiphate of copper has the same effect. The precipitate shows no trace of organization or crystallization; and it does not appear to differ in percentage composition from that of cellulose.

These same alkaline salts do not precipitate the solution of silk, and the fact may be made the basis of a process for separating silk from cotton. The solution of cellulose is precipitated also by alcohol, a concentrated solution of honey, gum Arabic, or dextrine. The cupro-ammoniacal liquid has no action on pyroxyline or collodion. Inuline, chitine, conchyoline, are insoluble in it.

Mr. Schlossberger has found that the ammoniacal hydrate of nickel, $NiO\,H^3N$, acts like the salt of copper. The solution of silk is however a fine blue in the latter and a yellowish brown in the former.

J. Nicklès on the diffusion of fluorine and the means of detecting it.—In this paper, the subject of which has been briefly presented in this Journal before, the following conclusions were arrived at.

1. There is fluorine in the blood, but less than has been supposed.

2. There are only small traces of fluorine in bones. After Berzelius, the proportion is 3 grams in 100 grams of the calcareous part of bones; but we have shown that there is hardly 0·05 in a kilogram.

3. The sources from which the animal organization may derive fluorine are: (*a*) potable waters; (*b*) vegetable substances,—although some contain so little that it is necessary to experiment on a kilogram at least of ashes, and on the products of evaporation of some thousand litres of water. Besides, some mineral waters are a source containing fluorine in even a large proportion—a fact that may explain the efficiency of certain mineral waters that are feebly mineralized, such as those of Plombières, and Mont d'Or, etc.

4. The water of the Seine taken at Paris, is one of those containing the least fluorine.

5. Of the rivers of France, one of the richest in fluorine is that of the La Somme near Amiens.

6. Mineral waters vary in amount of fluorine; the richest examined are, the waters of Contrexeville, Antogast, Rippoldsau, Geilnau and Châtenois (Bas-Rhin). Reactions may be obtained from a litre of these waters.

7. The Atlantic affords no sensible amount even from 300 litres, showing thus a striking difference between marine and mineral waters.

8. The law of the diffusion of fluorine may be thus expressed: *There is fluorid of calcium in all waters containing bicarbonate of lime, and therefore there may be fluorine in all rocks and minerals formed in a sedimentary way.*

9. There are two sources of error in the usual method of detecting fluorine—one arising from the fact that sulphuric acid alone will attack glass, and the other from the fact that this acid often contains small quantities of fluohydric acid.

10. These sources of error are eliminated from my methods—by using (*a*) quartz crystals in place of glass, and (*b*) sulphuric acid free from fluohydric acid.

11. The solvent which I use is chlorhydric acid, which, with a little care, may be found free from fluorine in the shops.

In the memoir I point out the circumstances under which such a chlorhydric acid may be produced in the manufacture in the large way.

On the Preparation of Ozone by von Babo, and by Messrs. Bunsen and Magnus.—The apparatus in which ozone is obtained by the combustion of phosphorus, permits of separating the gas from the phosphorous acid with which it is ordinarily mixed. This result is attained by causing the gas to pass through a solution of chromic acid. This acid not only oxydizes the phosphorous acid, but, as Baumert has shown, it increases the quantity of ozone; for after the washing there is more ozone than before, evidently because the oxydation of phosphorous acid is itself a cause of ozonization.

Von Babo has succeeded in drying ozone so far as to render it anhydrous, whence it follows that ozone, or at least this kind of ozone, cannot be confounded with the hydrogenated ozone HO^3 discovered by Baumert.

Bunsen and Magnus, who made remarks on this paper, expressed the opinion that we must admit two kinds of ozone, one allotropic oxygen and the other a hydrogenated compound.

Schönbein on Ozone.—See page 19 of this volume.

Notices by Prof. Erdmann, of Leipzig.—The name of Erdmann is in high regard among chemists, as well from his fine researches, as from his being the early teacher of the lamented Gerhardt.* Erdmann had the insight to detect the future greatness of this distinguished chemist and to open the treasures of his science to his pupil—so early deceased—whose labors have so greatly enlarged the horizon of chemistry.

It was our good fortune to make the personal acquaintance of the first master of our lamented friend, and to obtain from him information on the

* See our biog. notice of Gerhardt, this Jour., Jan., 1857, p. 102.

obscure points of his youth and his early scientific career. From this source we derive our knowledge of several new facts first established in his laboratory.

(1.) *Blistering principle of Ranunculus sceleratus.*—This principle occurs under the form of an acrid oil, which on the tongue is changed into a white mass of *anemonine* and *anemonic acid*. This transformation occurs in the plant during desiccation, but the vegetable then loses all its bitterness.

(2.) *Action of certain Metallic Salts on ligneous fibres.*—It is well known that to preserve wood, and particularly the ties of railways, it is usual to impregnate them with a solution of sulphate of copper. This salt combines with the fibre in a manner so intimate as to preserve it from the action of water, which has no effect to dissolve out the copper salt even when the prepared wood is submerged. This change happens only to wood in its natural state, for if the fibre is purified from albuminous matters, &c., although the copper salt appears to combine perfectly with it, on the least action of water it is dissolved out. Dilute solutions of sulphate of copper, in fact, remove the azotized substances from wood.

(3.) *Solubility of Sulphate of Baryta.*—This salt, one of the most insoluble of all substances in water, is soluble in water containing nitrate of ammonia, a concentrated solution dissolving sulphate of baryta in considerable proportion.

On new hydrocarbons and a new property of these bodies; by Mr. Fritzsche, of St. Petersburgh.—These hydrocarbons have been discovered in the tar resulting from the distillation of wood. They possess the peculiarity of forming beautiful and well defined crystalline compounds with picric acid, as well as those known of naphthaline and benzine. As the researches of Fritzsche have been some time published, we refer the reader to his memoir.

Manufacture of Soda and Baryta; by Mr. Kuhlmann.—The new facts established by Mr. Kuhlmann, of Lille, owe their discovery to a desire to render salubrious the manufacture of carbonate of soda by the process of Leblanc, which has heretofore been predjudicial to the public health, owing to the vast volumes of chlorhydric acid gas which have pervaded the atmosphere near such establishments. Mr. Kuhlmann has succeeded in avoiding this nuisance by the following process: He conducts the acid gases over masses of native carbonate of baryta, which arrest the hydrochloric acid, forming chlorid of barium. This salt, by means of dilute sulphuric acid, is changed into sulphate of baryta, which is now in great demand in the arts under the name of *blanc fixe* (permanent white). He manufactures 2000 kilograms per day.

Another new fact established by Mr. Kuhlmann relates to the economy of the residues of chlorid of manganese, resulting from the production of chlorine and hypochlorites. These residues retain a large quantity of chlorine, and Mr. Kuhlmann, who is one of the principal manufacturers, estimates the loss from this source to be not less than two million francs in France alone.

This skilful chemist has contrived two uses for these residues. Either he transforms them into chlorid of barium by means of carbon and sul-

phate of baryta, or he treats them by another residue which encumbers the soda-industry, viz., the oxysulphid of calcium, by which sulphid of manganese and chlorid of calcium result. The latter salt is now beginning to be used to prevent combustion and to water streets in summer, where by its hygroscopic properties it keeps down the dust.

All these researches were brought up in the first two sessions of the chemical section; but the third session was united with that of the physical section. On the next day there was an excursion to Baden Baden, when the Congress was fêted by that city. The Monday session was commenced at 11 o'clock, and there were many interesting experiments exhibited. The chief were—

Dove's Experiment in Acoustics.—This experiment consists in rendering the tone from a vibrating diapason, very distinct, so that it could be heard through the whole hall, by causing it to vibrate in a certain relation to a glass flask containing water. The flask should not be filled and the diapason should not touch it, but be held by the hand in the prolonged neck of the balloon. The sound returned depends on the position of the two limbs of steel to the neck of the flask. The perception of sound is most distinct when the plane of the two branches is in the axis of the neck, and it is null when this plane is perpendicular to the axis.

Dove ascertained these facts while engaged in researches as to the question whether the ear, which is for a time sensible to a certain tone, becomes insensible to it again, as the eye does to a given color when it has for some time contemplated it. The eye may be said to habituate itself to certain colors, as the olfactory nerves do to persistent odors. Dove's researches returned an affirmative reply to the point in question.

Magnus on the properties of iron in powder.—Metallic iron in a state of very fine division has for some years been used in medical practice. It is thus obtained when the oxyd of iron is reduced by hydrogen. When well prepared this form of iron is so combustible as to take fire on exposure to air, burning with scintillation. A manufactory has lately been established in the Tyrol for making iron-powder, of very considerable fineness—although the process is mechanical, consisting in using very fine files. Its therapeutic properties have not yet been decided. It does not burn spontaneously in air although it is extremely combustible, as the following experiment by Magnus demonstrated to the section. When a burning body is approached to these Tyrolean filings they do not inflame unless they are previously suspended from the poles of a magnet. It is an experiment easily repeated and interesting in a lecture. If a magnet be thus armed with these fine filings, and a flame applied, a combustion begins which spreads rapidly, and if the magnet is jarred a shower of burning particles fall through the air.

Boettger—Action of cold and warm water on horny substances.—This skillful experimenter whose tact in manipulation is well known, having obtained the floor of the Section, took a goose feather, placed it between the thumb and fore-finger of one hand and with the other crushed it into a crumpled mass. He then by a little manipulation restored it completely to its primitive state. The treatment by which this was accomplished was simple enough. After being left for some minutes in warm

water it was plunged into cold water; this restored the rigidity of the feather previously swollen by the warm water.

Schroeder—Relation between fermentation and crystallization.—In 1854 Mr. Schroeder in connexion with Mr. Dusch published a paper on fermentation and putrefaction, and showed that putrescent and fermentable substances could be indefinitely preserved, if instead of leaving such matter in common air, they were placed in vases filled with air that had been filtered through cotton. Flesh, soup, and all kinds of alimentary substances can thus be preserved, if the precaution has been taken previously to boil them in water.

Mr. Schroeder shows that what he has established concerning fermentation and putrefaction, is also true of crystallization. It is well known that a saturated solution of sulphate of soda remains liquid as long as it is in vacuo, but solidifies on access of air. Mr. Schroeder establishes the fact that crystallization does not take place if the air is made to pass through a tube filled with cotton.*

Mr. S. explained the results of his experiments in 1854 by supposing that the air filtered through cotton is deprived of the spores of cryptogamic infusoria, which are the cause of putresence and fermentation. If the experiment on the sulphate of soda tends to establish a relation between fermentation and crystallization, it serves to prove also that these phenomena can take place without the presence af these cryptogamia or infusorial germs, suspended in unfiltered air. This question which appeared to us finished by the earlier researches of Mr. Schroeder, comes up anew. These facts do not interfere with the mechanical theory of Liebig, nor that derived from the recent researches of Pasteur on the propagation of fermentation.

J. Nickles—Electromagnets and Magnetic adhesion.—The experiments on this subject have been reported briefly in former communications. They have acquired a new interest since the French Government has ordered General Morin, of the Department of Arts and Trades, to take up that part of my researches which is applicable to locomotion on railroads.

Before my investigations, only two kinds of magnets were known, the *straight* and the *horseshoe* or *bifurcate*.† In 1852 I made known the *trifurcate* magnet, (or magnet with three poles having only a single helix for magnetization although possessing considerable attracting power,) and the *paracircular* magnets,‡ and afterwards the *circular*.§ These last two kinds have some special properties, and are capable of transmitting motion as the revolution takes place, but the magnets which I call *circular* are polished at the circumference and without teeth. These magnets attracted much attention on account of their peculiarities and practical applications. One of them has been put in action on a large scale on the Lyons railroad.

This meeting of the German Association was without representatives from England, and but for the position of Carlsruhe would scarcely have had any from France. This is owing principally to the fact that Associations are in session in England, France, Italy and Germany at nearly the same

* Journal de Pharmacie and de Chemie, 1854, T. xxv, p. 314.

† This Journ., xv, 104, 880. ‡ Ibid, xvi, 110. § Ibid, xx, 99.

period of the year. The only way to remedy this difficulty is to substitute for these partial associations, a *European Scientific Association*, precursor to a *Universal Scientific Association*, which shall hold its sessions in turns at the different cities of the old and new continent.

Bibliography.—At H. Bossange's: *Researches on the Diffusion of Fluorine*, by J. Nicklès. 60 pp. 8vo.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the Siliciuret of Hydrogen.*—Wöhler has communicated a purely chemical method of preparing the siliciuret of hydrogen discovered by Buff and himself as a product of the electrolysis of an alloy of silicon and aluminum. The method in question was accidentally discovered in Wöhler's laboratory by Martins, who found that a scoria or slag arising from the preparation of magnesium by Deville's process, disengaged a spontaneously inflammable gas when treated with chlorhydric acid. The magnesium compound required in the preparation of the gas is prepared in the following manner: 40 grams of fused chlorid of magnesium, 35 grams of strongly dried fluosilicate of sodium, and 10 grams of fused chlorid of sodium are to be finely pulverized and intimately mixed in a hot mortar. The mixture is to be introduced into a glass vessel which can be closed, and 20 grams of sodium in very small pieces added. The whole is to be mixed by agitation, and then forced at once into a Hessian crucible, heated to redness. The crucible is to be covered and heated, when the combination takes place with repeated decrepitations. When these have ceased and flames of sodium no longer appear, the crucible is removed from the fire, allowed to cool, and broken. It contains a greyish-black fused mass filled with globules and plates resembling cast iron. The coarser pulverized mass is to be introduced into a flask with two tubulures, through one of which passes a funnel with a tube long enough to pass to the bottom of the flask, to the other tubulure is attached a short and wide conducting tube. The entire apparatus is now to be filled with boiled water, and then plunged beneath the surface of the pneumatic cistern, so that every bubble of air is expelled. A collecting tube may now be filled with water and inverted over the orifice of the tube conveying the gas. Strong chlorhydric acid is now to be poured through the funnel. A violent reaction ensues and much foam unavoidably passes over into the collecting tube with the gas; a second tube may, however, be filled with the gas without foam. The properties of the gas are as follows. Each bubble inflames on contact with air with a white flame and a violent explosion. The silicic acid formed produces beautiful rings like phosphuretted hydrogen. The gas is completely decomposed by a feeble red heat, brown amorphous silicon being deposited. When burned against a plate of porcelain it gives a brown spot. With chlorine the gas explodes violently, but not with protoxyd or deutoxyd of nitrogen. As thus prepared the gas still contains free hydrogen, which makes it difficult to determine its constitution. Siliciuret of hydrogen precipitates various metals from their solutions. A salt of copper agitated with the gas yields a red pellicle of a siliciuret of copper, which in the air oxyd-

izes to a lemon-yellow silicate of copper. Nitrate of silver yields with the gas a black substance which is doubtless a siliciuret of silver, mixed however with metallic silver: palladium is reduced by the gas to the metallic state. The greyish mass which yields the gas by the action of chlorhydric acid, appeared to consist of free silicon mixed with a siliciuret of magnesium which gives siliciuret of hydrogen by the action of chlorhydric acid, and of another siliciuret of magnesium which yields with chlorhydric acid free hydrogen and protoxyd of silicon. In one case the authors succeeded in isolating a lead-grey aggregate of regular octahedrons, sometimes presenting cubic surfaces. These were found to have the formula Mg_2Si, and as this compound yielded the spontaneously inflammable gas with chlorhydric acid, it is possible that the formula of this latter may be SiH_2. Martins is engaged in studying the subject further.—*Ann. de Chemie et de Physique*, liv, 218, Oct. 1858.

[Note.—It must be remembered that Wöhler and Martins take the equivalent of silicon as 21, so that silica is SiO_3. The siliciuret of magnesium above mentioned has no probable formula if we take silicon as 14, as appears necessary, since Marignac has shown the isomorphism of the fluosilicates and fluostannates. It is very much to be desired that those chemists whose means enable them to make such researches, should investigate the compounds of silicon with ethyl, methyl, &c. It can hardly be doubted that ethyl-zinc would give with chlorid or fluorid of silicon, a compound of ethyl and silicon having the formula $Si(C_4H_5)_2$ since we should have a reaction expressible by the equation

$$2\,.\,Zn(C_4H_5) + Si\,Cl_2 = 2Zn\,Cl + Si(C_4H_5)_2.$$

A determination of the density of the vapor of ethyl-silicon would possess much theoretical interest. The results obtained by Hoffmann and Cahours in the formation of compounds of ethyl, &c., with phosphorus and arsenic render the existence of similar compounds of silicon and boron almost certain.—W. G.]

2. *On protoxyd of iron with caustic potash as a reducing agent.*—Hempel finds that protoxyd of iron in the presence of an excess of caustic potash reduces iodic acid, bichlorid of platinum, and protochlorid of mercury. Platinum yields a black powder which after washing with water containing chlorhydric acid and drying, readily converts alcohol into acetic acid. A solution of chlorid of mercury treated with sulphate of iron and caustic soda, and then with sulphuric acid yields subchlorid of mercury and the filtrate is free from mercury. Nitrate and sulphate of protoxyd of mercury behave in this manner when a sufficient quantity of chlorid of sodium has been previously added. The author recommends this process for the determination of mercury, the precipitated calomel being collected on a weighed filter, washed and dried. To determine mercury volumetrically, Hempel gives the following process, which yields very good results. The solution of the chlorid, nitrate or sulphate of mercury (in the two last cases chlorid of sodium must be added,) is to be introduced into a capacious flask with a ground stopple, an excess of protosulphate of iron and caustic alkali added, the flask well shaken and the oxyd of iron dissolved by adding dilute sulphuric acid. The subchlorid of mercury is allowed to settle and the supernatant liquid filtered off. After complete washing the filter may be pierced and the contents washed down into the flask with the rest of the precipitate. A large excess of

dilute sulphuric acid and hypermanganate potash is then to be added, the flask closed and strongly shaken for two or three minutes. The undecomposed hypermanganate of potash is then to be removed by a solution of oxalic acid, and the excess of this last determined by means of a titred solution of the hypermanganate. The percentage of mercury is then easily calculated.—*Ann. der Chemie und Pharm.* cvii, 97.

3. *On the Iodid of Methylen.*—When powdered iodine is added to crystallized ethyloxyd-soda a strong reaction occurs, and the mass becomes fluid. By distilling the mass Buttlerow obtained a heavy oily substance dissolved in alcohol and precipitated from this by water: this is the iodid of methylen $C_2H_2I_2$. The same substance is formed in larger quantity when one eq. of iodoform C_2HI_3 is added to three eqs. of ethyl-oxyd-soda, and water added to the product of this reaction. The iodid is a heavy oily liquid of a yellowish color and of density 3·342: at 2° it solidifies to a crystalline mass. By heating the iodid with acetate of silver and a little crystallizable acetic acid to 100° extracting the mass with ether and then distilling, a colorless oily liquid passed over at about 170°. This is the acetate of methyl-glycol $\left.\begin{matrix} C_2H_2 \\ (C_4H_3O_2)_2 \end{matrix}\right\} O_4$. The author did not however succeed in obtaining methyl-glycol from this body.—*Ann. der Chemie und Pharmacie*, cvii, 110.

4. *On the action of Ammonia upon Glyoxal.*—By the action of nitric acid upon alcohol Debus obtained two new bodies termed respectively glyoxal $C_4H_2O_4$ and glyoxylic acid $C_4H_4O_6$. By the action of a warm and concentrated solution of ammonia upon glyoxal, Debus obtained a base having the formula $C_{12}H_6N_4$, its formation being represented by the equation

$$3(C_4H_2O_4) + 4NH_3 = C_{12}H_6N_4 + 12HO.$$

Glycosin the new base is a light white powder soft like talc and without taste or smell: it sublimes without melting, and yields beautiful needles. With bichlorid of platinum it yields a beautiful yellow crystalline powder, which has the formula $C_{12}H_6N_4 + 2(HCl, PtCl_2)$ or $C_{12}H_8N_4Cl_2, 2PtCl_2$. The rational formula of glycosin according to Debus is

$$N_4 \left\{ \begin{matrix} C_4H_2 \\ C_4H_2 \\ C_4H_2 \end{matrix} \right\}.$$

The mother-liquor from which the glycosin is obtained yields with oxalic acid the oxalate of a new base which the author terms glyoxalin, and which has the formula $C_6H_4N_2$. Glyoxalin is colorless and crystalline; its platinum salt has the formula $C_6H_4N_2, HCl + PtCl_2$ and crystallizes in magnificent orange-red prisms. The formation of glyoxalin is expressed by the equation

$$2(C_4H_2O_4) + 2NH_3 = C_6H_2N_2 + C_2H_2O_4 + 4HO.$$

Ann. der Chemie und Pharmacie, cvii, 199.

5. *On the constitution of Tantalite.*—H. Rose has published an elaborate discussion of the analyses of tantalite from different localities, and arrives at the conclusion that the formula of the unaltered mineral is $FeO, 2TaO_2$, a portion of the tantalic acid being replaced by stannic acid SnO_2 and zirconia, which latter has probably the formula ZrO_2, as Deville and Troost have suggested.—*Pogg. Ann.*, civ, 85.

6. *On Niobium.*—H. Rose has published in part, the results of his long continued and elaborate investigations of niobium and its compounds—investigations which may justly be considered as among the most difficult and tedious which chemists have ever undertaken. We shall content ourselves with a brief abstract of the most important points in the history of the metal.

Metallic niobium is most easily prepared by heating the double fluorids or hypofluorids of niobium and the alkaline metals with sodium to a strong red heat in a crucible of cast iron. After cooling, the black mass is to be diffused in cold water in a platinum capsule; the metallic niobium boiled with water, and finally washed with water containing a little alcohol, till the washings leave no residue on evaporation. The metal obtained is purer, when a tolerably thick layer of chlorid of potassium is placed upon the mixture of fluorids with sodium before ignition. Metallic niobium is a black powder which conducts electricity, and is acted on by reagents more easily than tantalum. Freshly prepared and still moist niobium when heated with dilute chlorhydric acid is dissolved with evolution of hydrogen. The colorless solution gave with ammonia a voluminous precipitate of a brownish color, which however oxydized upon the filter and became white. It is therefore clear that there exists a stage of oxydation of niobium which is lower than hyponiobic acid. Nitric acid does not dissolve niobium even on heating. Concentrated sulphuric acid dissolves metallic niobium by long heating, the solution has a brownish color and gives a brownish precipitate with ammonia. Fluohydric acid also dissolves niobium, and the solution is effected still more easily by a mixture of sulphuric and fluohydric acids. Fusion with carbonate of potash and boiling with caustic potash, also dissolve the metal. When heated in chlorine the metal ignites, both the yellow and the white chlorid being formed—the latter, Nb_2Cl_3, in large excess. This latter cannot be converted into the yellow chlorid $NbCl_2$ by heating in chlorine. The oxydation of niobium yields only hyponiobic acid Nb_2O_3, and not niobic acid NbO_2, so that in this respect the metal differs from tantalum. The density of the metal obtained from the fluorids was 6·297; of that obtained from the yellow chlorid by means of sodium, 6·272, but the density varied greatly in different specimens in consequence of the presence of more or less hyponiobic acid as impurity. When phosphorus vapor is passed over bi-hyponiobate of soda, heated to redness in a current of hydrogen, the metal is reduced and contains only a trace of phosphorus: this reduction takes place much less easily and completely in the case of tantalum. In his second memoir Rose treats of the chlorids of niobium. The yellow chlorid, $NbCl_2$, resembles the corresponding chlorid of tantalum, $TaCl_2$, but has a clearer and somewhat deeper color; it is also more volatile than the latter, beginning to pass over at 125° C., while chlorid of tantalum becomes gaseous at about 144° C. The chlorid of niobium melts at 212° C., and solidifies sooner than the chlorid of tantalum, which fuses at a rather higher temperature. Rose made repeated analyses of the chlorid of niobium, decomposing it with water, and determining the chlorine and niobic acid produced. These analyses from the extreme difficulty of the subject did not yield results which correspond as accurately as could be desired. The author rejects the results of the first five, and

from the mean of the last three deduces the numbers 48·82 (or 610·37 O = 100) as the equivalent of niobium.

Chlorid of niobium, $NbCl_5$, dissolves in chlorhydric acid; after some time the solution becomes turbid and gelatinizes. Water does not completely dissolve the mass, the filtrate is opalescent, and contains much niobic acid, which may however be almost completely separated by boiling. When, however, the chlorid is boiled with chlorhydric acid, a turbid solution is produced, which does not gelatinize, and forms with water a clear solution which is not precipitated by boiling. The chlorid dissolves in alcohol to form a clear solution, while a small quantity remains which gelatinizes with water. When the alcoholic solution is distilled, alcohol, chlorid of ethyl, and finally, chlorhydric acid pass over, while a syrupy liquid remains which dissolves in water, giving a clear solution from which nothing is precipitated by boiling. The syrupy liquid is doubtless niobate of ethyl. When chlorid of niobium is dissolved in chlorhydric acid, water added, and then metallic zinc placed in the solution, a beautiful blue color is produced. Bromine forms two compounds with niobium, one of which is yellowish and voluminous, and corresponds to the hypochlorid, while the other is purple-red, but becomes yellow on strong heating and volatilizes. The yellow color of the hypobromid and the red of the bromid appear to be due simply to the presence of free bromine.

In a third memoir the author treats of the fluorids of niobium. The hydrate of niobic acid dissolves readily in fluohydric acid, and the solution gives a series of crystallized double fluorids. The potassium salts are colorless and crystalline. Of these, one has the formula $KF + NbF_2$, while the other is $(KF + NbF_2) + (KF + HF)$. The soda salts are $NaF + NbF_2$, $(2NaF + NbF_2) + (NaF + HF)$, and $(NaF + NbF_2) + (NaF + HF)$. It is difficult however to obtain these salts in a state of purity.—*Pogg. Ann.*, civ, 310, 432, 581.

7. *On the constitution of titaniferous iron ores.*—Rammelsberg has published an elaborate investigation of the titaniferous iron ores, the principal results of which are as follows:

(1.) The greater number of the titaniferous iron ores, among them all the crystallized forms, consist of 1 eq. of titanic acid and 1 eq. of protoxyd of iron (prot. of manganese or magnesia).

(2.) Magnesia is an essential constituent of all these ores. In the crystallized mineral from Layton, the magnesia amounts to 14 per cent.

(3.) According to Mosander's theory the titaniferous iron ores are either simply titanates of protoxyd of iron $FeTi$; with isomorphous admixtures of titanate of magnesia or mixtures of such with sesquioxyd of iron, for the most part in simple proportions.

(4.) The theory of H. Rose that these ores consist of isomorphous sesquioxyds of titanium and iron, would require the assumption of a sesquioxyd of magnesium.

(5.) The author prefers Mosander's theory for the present state of our knowledge.

(6.) In Iserin we find grains consisting of $FeTi$, and $FeTi_3$.

(7.) No titaniferous iron crystallizing in regular octahedrons is known. The dense masses or octahedral grains which contain titanium appear to be mixtures.

(8.) The crystallized magnetic iron ores contain no titanium, they consist of one atom of protoxyd and one atom of sesquioxyd.

(9.) All the Elba iron ore does not contain titanium, but all, like that from Vesuvius, contains magnesia and protoxyd of iron.

(10.) The strongly magnetic octahedrons from Vesuvius, hitherto considered as a specular iron, which are accompanied by rhombohedrons of specular iron, contain in part large quantities of magnesia, and in part protoxyd of iron. They consist either of magnetic iron which has been partially converted into sesquioxyd of iron, as well as of the isomorphous combination $MgFe$, or, as is more probable, the two protoxyds are isomorphous with sesquioxyd of iron, which is itself dimorphous.—*Pogg. Ann.*, civ, 497.

8. *On a new acid obtained by the oxydation of malic acid.*—By the action of bichromate of potash upon a dilute solution of malic acid, Dessaignes has obtained an acid which has the formula $C_6H_4O_8$, and which may possibly be identical with the nicotic acid of Barral. The author terms it provisionally malonic acid, and remarks that it is probably homologous with oxalic acid, being the term hitherto wanting between oxalic and succinic acids. Malonic acid forms large rhombohedral crystals, and is easily soluble in water and alcohol. It has a strongly acid taste, melts at 140°, and is decomposed at 150°. By dry distillation it yields a mixture of acetic acid with unchanged malonic acid; carbonic acid is set free at the same time, the equation being

$$C_6H_4O_8 = C_4H_4O_4 + C_2O_4.$$

Malonic acid forms neutral and acid salts with the alkalies. Malonate of ammonia precipitates the salts of lime, baryta, silver and mercury. The author remarks that while the analogy between malonic and oxalic acids is strongly marked, the resemblance between malonic and succinic acids is much less distinct.—*Comptes Rendus*, xlvii, 76. W. G.

9. *Remarks on Chemical Science;* by Sir JOHN HERSCHEL, at the recent meeting of the British Association at Leeds.—Since organic chemistry has assumed, by the experiments and reasonings of Dumas, Liebig, Hoffmann, and its other distinguished cultivators, that highly abstract and intellectual form under which it now presents itself, and which by the links of the platinum bases, and compounds such as those described by Gibbs and Genth, under the name of the ammonio-cobaltic bases, and by those which are every day coming into view by the mutual interweaving if I may use such an expression, of the organic and inorganic systems of composition in bases such as those of the metallic ethyls and those of boron and silicon, it seems to place these conceptions in much the same sort of relation to the ordinary atomic theory as put forth by Dalton and Higgins, and the elementary notions of oxyd, acid, and base of Lavoisier, that the transcendental analysis holds to common algebra. And here perhaps I may be tolerated if I put in a word of reclamation against the system of notation into which chemists who for the most part are not algebraists, have fallen, in expressing their atomic formulas. These formulas have been gradually taking on a character more and more repulsive to the algebraical eye. There is a principle which I think ought to be borne in mind in framing the conventional notations, as well as nomenclatures of every science, at every new step in its progress, viz: that as sciences do

not stand alone, but exist in mutual relation to each other—as it is for their common interest that there should exist among them a system of free communication on their frontier points—the language they use and the signs they employ should be framed in such a way as at least not to contradict each other. As the atomic formulas used by the chemist are not merely symbolic of the mode in which atoms are grouped, but are intended also to express numerical relations, indicative of the aggregate weights of the several atoms in each group and the several groups in each compound, it is distressing to the algebraist to find that he cannot interpret a chemical formula (I mean in its numerical application) according to the received rules of arithmetical computation. In a paper which I published a long time ago on the hyposulphites, I was particularly careful to use a mode of notation which, while perfectly clear in its chemical sense, and fully expressing the relation of the groupings I allude to, accommodated itself at the same time perfectly well to numerical computation, no symbol being in any case juxtaposed, or in any way intercombined with one another, so as to violate the strict algebraic meaning of the formula. This system seemed for a while likely to be generally adopted, but it has been more and more departed from, and I think with a manifest corresponding departure from intelligibility.

The time is perhaps not so very distant when from a knowledge of the family to which a chemical element belongs, and its order in that family, we may be able to predict with confidence the system of groups into which it is capable of entering, and the part it will play in the combination. A great step in this direction seems to me to have been lately made by Prof. Cooke of Harvard University, in the United States, (in a memoir which forms part of the 5th volume of the Memoirs of the American Academy of Arts and Sciences,) to extend and carry out the classification of chemical elements into families of the kind I allude to, in a system of grouping, in which the first idea, or rather the first germ of the idea, may be traced to a remark made by M. Dumas, in one of his reports to this Association, and which is founded on the principle of arranging them in a series, in each of which the atomic weight of the elements it comprises are found among the terms of the arithmetical progression, the common difference of which in the several series are 3, 4, 5, 6, 8, and 9 times the atomic weight of hydrogen respectively. So arranged they form six groups, which are fairly entitled to be considered natural families, each group having common properties in the highest degree characteristic; and what is more remarkable, the initial member in each group possessing in every case the characteristic property of the group in its most eminent degree, while the others exhibit that property in a less and less degree, according to their rank in the progression, or according to the increased numerical value of the atomic equivalent. Generally speaking, I am a little slow to give full credence to numerical generalizations of this sort, because we are apt to find their authors either taking some liberties with the numbers themselves, or demanding a wider margin of error in the application of their principles than the precision of the experimental data renders it possible to accord, so that the result is more or less wanting in that close appliance to nature which makes all the difference between a loose analogy and a physical law; but in this instance it certainly does

appear that the groups so arising not only do correspond remarkably well in their theoretical numbers with those which the best authorities assign to their elements, but that it really would be difficult to distinguish the elements themselves into more distinctly characteristic classes, by a consideration of their qualities alone, without reference to their atomic numbers. When we find, for instance, that the principle affords us such family groups as oxygen, fluorine, chlorine, bromine, and iodine, self-arranged in that very order; or again, nitrogen, phosphorus, arsenic, antimony, and bismuth; when we find that it packs together in one group all the more active and soluble electro-positive elements, hydrogen, lithium, sodium, and potassium, and in another the more inert and less soluble ones—calcium, strontium, barium, and lead—and that without outraging any other system of relations, it certainly does seem that we have here something much like a valid generalization: and I shall be very glad to learn in the course of any discussions which may arise on such matters as may be brought before us in the regular conduct of our business from those more competent to judge than myself, whether I have been forming an overweening estimate of the value and importance of such generalizations.

I will only add on this point, in reference to what fell from our excellent President in his address to the assembled Association last night, that this kind of speculation followed out would seem to me likely to terminate in a point very far from that which would regard all the members of each of these family groups as allotropes of one fundamental one, inasmuch as the common difference of the several progressions which their atomic weights go to make up, are neither equal to nor in all cases commensurate with the first terms of these progressions. For instance, in the chlorine group, the first term being 8, the common difference is 9. Something very different from allotropism is surely suggested by such a relation. It would rather seem to point to a dilution of energy of one primary element by the superaddition of dose after dose of some other modifying element, and this the more strikingly since we find oxygen standing at the head of very distinct groups having very striking correspondence in some respects, and very striking differences in others. But all these speculations take for granted a principle, with which I must confess I think chemists have allowed themselves to be far too easily satisfied, viz: that all the atomic numbers are multiples of that of hydrogen. Not until these numbers are determined with a precision approaching that of the elements of the planetary orbits, a precision which can leave no possible question of a tenth or a hundredth of a per cent, and in the presence of which such errors as are at present regarded as tolerable in the atomic numbers of even the best determined elements shall be considered utterly inadmissible, I think can this question be settled—and when such gigantic consequences—so entire a system of nature is to be based on a principle—nothing short of such evidence ought, I think, to be held conclusive, however seductive the theory may appear. I do not think such precision unattainable, and I think I perceive a way in which it might be attained, but one that would involve an expenditure of time, labor, and money, such as no private individual could bestow upon it. If the phenomena of chemistry are ever destined to be reduced under the

dominion of mathematical analysis, it will no doubt be by a very circuitous and intricate route, and in which at present we see no glimpse of light. We should therefore be all the more carefully on the watch in making the most of those classes of facts which seem to place us, not indeed within view of daylight, but at what seems an opening that may possibly lead to it.

Such are those in which the agency of light is concerned in modifying or subverting the ordinary affinities of material elements, those to which the name of actino-chemistry has been affixed. Hitherto the more attractive applications of photography have had too much the effect of distracting the attention from the purely chemical questions which it raises, but the more we consider them in the abstract, the more strongly they force themselves on our notice, and I look forward to their occupying a much larger space in the domain of chemical inquiry than is the case at present. That light consists in the undulations of an etherial medium, or at all events agrees better in the characters of its phenomena with such undulations, than with any other kind of motion which it has been possible to imagine, is a proposition on which I suppose the minds of physicists are pretty well made up. The recent researches of Professor Thomson and Mr. Joule moreover have gone a great way towards bringing into vogue, if not yet fully unto acceptation, the doctrine of a more or less analogous conception of heat. When we consider now the marked influence which the different calorific states of bodies have on their affinities—the change of crystalline form effected in some by a changing temperature—the allotropic states taken on by some on exposure to heat—or the heat given out by others on their restoration from the allotropic to the ordinary form (for, though I am aware that Mr. Gore considers his electro-deposited antimony to be a compound, I cannot help fancying that at all events the state in which the the antimony exists in it is an allotropic one); when, I say, we consider these facts in which heat is concerned, and compare them with the facts of photography, and with the ozonization of oxygen by the chemical rays or the electric spark, and with the striking attractions in the chemical habitudes of bodies pointed out by Draper, Hunt, and Becquerel; and when again we find these carried so far that, as in the experiments of Bunsen and Roscoe, we find the amount of chemical action numerically measuring the quantity of light absorbed, it seems hardly possible not to indulge a hope that the pursuit of these strange phenomena may by degrees conduct us to a mechanical theory of chemical action itself. Even should this hope remain unrealized, the field itself is too wide to remain unexplored; and, to say nothing of discovery, the use of photography merely as a chemical test may prove very valuable, as I have myself quite recently experienced, in the evidence it has afforded me of the presence in certain solutions of a peculiar metal having many of the characters of arsenic, but differing from it in others, and strikingly contrasted with it in its powerful photographic qualities, which are of singular intensity, surpassing iodine, and almost equalling bromine.

There is another class of phenomena which, though usually considered as belonging peculiarly to the domain of general physics, and so out of our department, seems to me to want some attention in a chem-

ical point of view. It is that of capillary attraction. The co-efficient of capillarity differs very remarkably in different liquids, and no doubt also in their contacts with different solids, a fact which can hardly be separated from the idea of some community of nature between the capillary force and those of elective attraction. I hardly dare to hint at the existence of some slight misgiving I have always felt as to the validity of the received statical theory of capillary action which carries with it the authority of such names as those of Laplace and Poisson. Any discussion of this point would be matter for another section of this Association, and if I here touch upon it, it is only to observe my impression of the requisiteness of a force so far allied to chemical affinity as to be capable of saturation, rests on other grounds besides that of the mere diversity of action above alluded to. But I must remember that you are not met here to listen to generalities of whatever nature, but that we have plenty of real and special business before us.

10. *An account of some experiments on Radiant Heat, involving an extension of Prévost's Theory of Exchanges;* by Mr. B. Stewart, (Proc. Brit. Assoc., Ath. 1614).—These experiments were performed with the aid of the thermomultiplier, the source of heat being for the most part bodies heated to 212°. Four groups of experiments were considered. Group the first contains those experiments in which the quantities of heat radiated from polished plates of different substances at a given temperature, are compared with the quantity radiated from a similar surface of lampblack at the same temperature. The result of this group of experiments is, that glass, alum and selenite, radiate about 98 per cent of what lampblack does—thick mica, 92—thin mica, 81—and rock salt only 15 per cent. The second group of experiments was designed to compare together the quantities of heat radiated at the same temperature from polished plates of the same substance, but of different thicknesses. The result of this group was, that while the difference between the radiating power of thick and thin glass is so small as not to be capable of being directly observed, there is a perceptible difference between the radiation from thick and thin mica, and a still more marked difference between the radiation from plates of rock salt of unequal thickness. The third group of experiments was made with the view of comparing the radiations from various polished plates with that from lampblack, as regards the quality of the heat,—its quality being tested by its capability of transmission through a screen of the same material as the radiating plate. From this group of experiments it appears that heat emitted by glass, mica, or rock salt is less transmissible through a screen of the same material as the heated plate than heat from lampblack,—this difference being very marked in the case of rock salt, which only transmits about one third of the rays from heated rock salt. The common opinion that rock salt is equally diathermanous for all descriptions of heat is therefore untenable. The fourth group of experiments shows that heat from thick plates of glass, mica, or rock salt is more easily transmitted by screens of the same nature as the heated plate than heat from thin plates of these materials. It was shown that all these experiments may be explained by Prévost's theory of exchanges, somewhat extended. This extension consists of the following laws:—1. Each particle of a substance has an independent

radiation of its own equal in all directions and without regard to the distance of the particle from the surface of the body. 2. The radiation of a particle equals its absorption, and that for every description of heat. 3. The flow of heat from within upon the interior surface of a polished plate of indefinite thickness is proportional to the index of refraction of the body, and that for every description of heat.

The bearing of these experiments on Dulong and Petit's law of radiation was then attempted to be traced. It was shown that unless bodies from simply being heated change their transmissibility for the same description of heat (which there is no reason to suppose), the radiation of thin plates or particles at a high temperature will bear a less proportion to the total radiation of that temperature than at a low,—the consequence will be, that the radiation of single particles will increase with the temperature in a less degree than Dulong and Petit's law would indicate. It may even be that the radiation of a particle or very thin plate may be proportional to the absolute temperature of that particle. Taking a piece of glass or mica, therefore, at a low temperature, as it is very opaque with regard to the heat radiated by itself, we may suppose that the total radiation consists of that of the outer layer of particles only, that from the inner layers being all stopped by the outer. At high temperatures, however, we may suppose that there is not only the radiation of the outer layer, but also part of that of the inner layer which has been able to pass, swelling up the total radiation to what it appears in Dulong and Petit's experiments. This way of looking at radiation may possibly bring the radiative power of particles to obey the same laws with the conducting power of particles, which Prof. Forbes has shown decreases with an increase of temperature. The author of this communication is indebted to Prof. Forbes for the use of the instruments and substances employed, and also for many valuable suggestions with regard to the experiments it contains.

11. *On the Phosphorescent Appearance of Electrical Discharges in a Vacuum made in Flint and Potash Glass;* by Mr. J. P. Gassiot, (Proc. Brit. Assoc. Ath., 1615).—The discharge from an induction coil when taken in a vacuum tube made of flint glass, has (under certain conditions) the property of rendering the glass highly phosphorescent, the phosphorescence being denoted by the intense blue color of the glass with which the stratifications are surrounded. On trying the discharge in some vacuum tubes I had obtained from Mr. Geissler, of Bonn, I observed that the phosphorescence was no longer blue, but was of a slight green color. To test whether this difference was due to the gaseous matter remaining in Geissler's tubes, or to the character of the glass which he uses, I had Torricellian vacuums prepared in German glass tubes, and in this manner ascertained that the difference in the color was entirely due to the character of the glass: that of Germany is, I believe, made with potash, and is entirely free from any lead, while in the English flint glass lead is introduced to some extent. I have recently obtained a vacuum tube from Bonn, which shows this difference in a very beautiful manner: the outer ends of the tube are composed of German glass, the centre of the tube is of English glass; by this arrangement the contrast between the two is very manifest.

12. *On Induced Electric Discharges when taken in Aqueous Vapor;* by Mr. J. P. Gassiot, (Proc. Brit. Assoc. Ath., 1615).—If the tube of a well constructed water-hammer is partly covered with two separate coatings of tin-foil, and the coatings are connected one with the outer, and the other with the inner terminal of an induction coil, a discharge will be observable through the centre of the tube in the form of a wave line. On repeating this experiment I ascertained that the vacuum in the tube was very much deteriorated. I could no longer produce that peculiar bubbling in the ball of the apparatus which is always attainable by gently heating the tube with the warmth of the hand; this bubbling was originally very sensibly perceptible in the tube I now exhibit when I first received it from the maker, Mr. Casella. I have repeated the experiment with other water-hammers, and always with the same result; but I have not yet opened one to examine whether the vapor has been decomposed, and gas evolved.

II. GEOLOGY.

1. *On Marcou's "Geology of North America;"* by Prof. Agassiz.—I have not yet seen Marcou's latest publication on American Geology, but I have now open before me, his paper in the Proceedings of the Geological Society of France, and that in Petermann's "Geographische Mittheilungen," both bearing date 1855, as well as the Geological Map of the United States and British North America by H. D. Rogers, also bearing date 1855, and Hall's and Leslie's Map of the country west of the Mississippi river, published with the 1st vol. of Emory's Report in 1857. I take it that it will be no injustice to either Rogers or Hall to go to an earlier publication of Marcou's, in a comparison of their respective claims to correct illustration of our Western Geology. Let me premise by saying that as far as the geology of the East is concerned, from Iowa to the Atlantic coast, I acknowledge that to Hall is due, unquestionably, the credit of having settled by extensive comparisons, and by personal examinations, the true geological horizon of the vastest extent of our continent, not only by an examination of the superposition of the rocks, but also by the most minute and most extensive study of the fossils.

We all know also how much the Rogerses have done to elucidate the physical geography, the orography, and the order of succession of the formations of Pennsylvania and Virginia, which has thrown much light upon the general geology of the eastern part of the continent. It is equally well known how much the special state surveys have added to the details in this general investigation of the Geology of North America. But when we go west of the Mississippi valley to the Pacific shores the case is very different. The maps of Rogers, Hall and Marcou, are a compilation and an attempt at coordination of surveys which cover only a very small portion of the ground. They are, as it were, the reading of the authors of these different maps, of investigations made by others, though Marcou has here unquestionably the advantage of having gone himself over the ground.

A comparison for instance, of the manner in which the volcanic rocks are dotted over New Mexico, Sonora, and Lower California, as well as in California, Oregon and Washington Territories by Hall and Rogers,

with Marcou's representation of the same cannot fail to show to a geological reader, that they are more natural in Marcou's map than in the two others. When a region is not more minutely surveyed than the whole western half of our continent, of which we have not even accurate geographical maps, it is not possible to expect accuracy in detail, and the critic must consider the general connection rather than special points.

I do not see, for instance, how the omission of State boundary lines which, in a former review of Marcou's map in the Journal, was made a prominent objection to his representation of American geology, can be of any importance in such a general survey of the subject. Rogers in his map does not give these boundaries any more than Marcou.

But I now come to the essential point. What is the true geological character of those five hundred thousand square miles of land, extending between the Mississippi, west of Arkansas and Missouri, and the great Salt Lake Basin? Rogers colors it uniformly with Cretaceous rocks, and the well known Tertiary deposits, adding metamorphic rocks, flanked with Carboniferous in the mountainous tracts. Hall does the same only making in addition, a distinction between the upper and lower Cretaceous, while Marcou distinguishes further between Permian, Triassic and Oolitic beds. I do not suppose that he, any more than Hall and Rogers, imagines that the boundaries he assigns to any of these groups are any more accurate than those assigned by Rogers and Hall to the groups they distinguish. These appear to me simply in the light of the respective readings of isolated facts recorded in the way they have struck the authors of these different maps. When in his paper to the Geological Society of France, Marcou speaks of himself as a travelling geologist who "brings his little stone to the great edifice" (page 3) it does not appear to me as vain-glorious boasting, and we ought to take gratefully the contributions of a Frenchman, using language after the fashion of his nation, even though it be not the way in which we would have expressed ourselves. Now I confess that after reading the condensed Review of American Geology which Marcou has given, in Petermann's Contributions, I find in it a more comprehensive account of the general features of the orography and geology of the Western half of our continent, than in the other representations I have read upon this subject. I think that even now a translation of that paper would be welcome to every English student of American geology, and that far from circulating false impressions, it would greatly contribute to bring before the mind the grand features of that remarkable country, and to connect in an intelligible way the geology of the West with that of the East. The middle tract of our continent is unquestionably occupied by deposits younger than the coal; I do not allude to the Lake Superior Sandstone respecting which I believe Marcou to be mistaken,—but the five hundred thousand square miles of questionable character as to the details, certainly belong to those from recent formations.

Now it appears to me that the geology of our Atlantic States furnishes data upon which theoretical inferences, bearing upon the question which Marcou's assertions call forth, may be founded. We know that the Cretaceous formations extend from the Atlantic slope of the Alleghany range round their southern spur into the great geological gulf

now occupied by the Mississippi valley. We know further that along the eastern slope of the Alleghanies, beginning with the Connecticut valley, there extends, between the axis of elevation of that chain and the Cretaceous deposits at its Atlantic foot, a series of deposits referred respectively to the Triassic and the Oolitic series.

We know also that to the south of North Carolina, these lower secondary deposits are covered over by the Cretaceous. Now, since the upheaval of the Alleghanies is anterior to the deposition of the Trias, does it not appear natural to suppose that Triassic and Oolitic formations must have been deposited at the foot of the western slope of the Alleghanies as well as upon its eastern slope, and that the Cretaceous deposits overlap them in the Mississippi gulf in various ways, as along the Alleghany chain, and that, following various routes, the different geologists who have gone across the continent must have seen, here Trias, then Jura, and then again Cretaceous beds, overlaid by Tertiaries, in a number of points, already determined, though the relative extent of all these beds, over a surface of 500,000 square miles, remains yet to be ascertained.

The circumstance that Marcou has colored in yellow the whole middle tract of the continent, can express nothing but his conviction that the whole Mississippi gulf is lined with Triassic beds, overlaid with more or less extensive Jurassic, Cretaceous and Tertiary deposits. In such a theoretic representation of the geological features, where the details are wanting, provided the existence of the Trias and Jura is made out somewhere, there is no more inaccuracy than in coloring a map of our eastern geology, where the drift covers the greatest extent of the surface, as if it were altogether occupied by Palæozoic rocks.

I take it that such things are, by this time, understood by all those who examine schematic maps,—at least they should be. Moreover, the discoveries by Professor Swallow and Mr. Meek of Permian beds in Kansas, along the eastern border of the great Mississippi gulf, and by Professor Hall in Iowa, furnish a very unexpected confirmation of the broad statement first made by Marcou, that while the Eastern part of our continent consists of Palæozoic rocks, the middle part is occupied by the Mesozoic series. I truly believe that, at some future period, the general outline of our western geology by Marcou, which by the way, has the priority over the others, will stand before a complete survey of the whole in the same light as Maclure's old map now stands, when compared to the well-known eastern geology.

In this connection, I cannot but remember that, with Thurmann, Mandelslohe, Gressly, Quenstedt, Römer, d'Orbigny and Oppel, Marcou is one of the geologists who knows the Jurassic formation best; that he has published a masterly paper upon the Jura Salinois in the Transactions of the Geological Society of France; and that it seems hardly credible to me that he should have been so completely mistaken in his identification of Oolitic beds in the west. I have myself, in my collection, a large number of specimens of the Cretaceous fossils of Texas and of New Jersey, among which is a beautiful series of the Exogyra, characteristic of the Cretaceous period, and I have seen the Exogyra and the Ostrea which Marcou brought from his excursion across the continent, and I distinctly remember that I could not identify them with the Cretaceous species, but rather thought them allied to Jurassic species.

Whoever has read Marcou's paper on the Jura must have seen that he knows, as well as any geologist living, that lithological characters are of no value in identifying geological horizons. But after having presented the general evidence, as far as it goes, for the presence of Triassic and Oolitic beds in the middle tract of our continent, I cannot find that there is any reason for blame, with his familiarity with the Triassic and Oolitic rocks of Europe, in his pointing out the lithological resemblance there may be between them, any more than there is ground for blaming the American geologists who, after identifying certain beds in New Jersey as Cretaceous, have also alluded to their mineralogical resemblance with the Green Sand of Europe; for this is, after all, a remarkable fact which runs over immense tracts of geological deposits belonging to the same horizon.

Reply to Prof. Agassiz on Marcou's Geology of North America, by JAMES D. DANA.—I regret in such a case as this to have to differ from Professor Agassiz. The amount of difference is however not as great as at the first reading may appear; for an important part of the positions in my paper are untouched, and an explicit dissent from some of the views of Mr. Marcou is expressed.

The statements in Professor Agassiz's remarks to be especially noted are the following:

1. That Professor Agassiz had not read the work reviewed, but had seen the earlier papers by Mr. Marcou and examined his geological map.

2. That while, as regards the geology of the East from Iowa to the Atlantic coast, "to Mr. Hall is due unquestionably the credit of having settled by extensive comparisons and by personal examinations the true geological horizon of the vastest extent of our continent, not only by examination of the superposition of the rocks, but also by the most minute and most extensive study of the fossils;" and that while the "Professors Rogers have done much to elucidate the physical geography, the orography, and the order of succession of the formations of Pennsylvania and Virginia, and have thrown much light upon the general geology of the eastern part of the Continent,"—west of the meridian of Iowa their observations have not extended, and Marcou has thence the advantage of them.

3. That the maps of the region west of the Mississippi by Rogers, Hall, and Marcou are mainly compilations from the results of various surveys, and that Marcou in extending the colors of the Triassic formation over the 500,000 square miles of the Rocky mountains, and laying down also the Permian and Jurassic over the same region, was no more culpable than Hall or Rogers in covering it with Cretaceous.

4. That Marcou is mistaken in regarding the Lake Superior Sandstone as Triassic.

5. That it is hardly credible that Mr. Marcou should have been so completely mistaken in his identification of Oolitic beds in the west; and that the two species collected by Marcou from the beds are most allied, in Professor Agassiz's opinion, to Jurassic species.

6. That Mr. Marcou knows that lithological characters are of no value in identifying geological horizons; and that adding these characters to other general evidence for the Triassic and Oolitic rocks is not blameable.

The claims which Mr. Marcou has put forward in his work are: (1) the correct determination of the Red Sandstone of the Lake Superior region;

(2) the identification, for the first time, of the Permian over the Rocky Mountain region; (3) the same, of the Triassic; (4) the same, of the Jurassic. I have presented evidence proving, as I believe, that he was wrong in each case; and hence, that the claims of prediscovery which he is now urging over Europe are groundless. Besides this, I have pronounced the work abusive of such men as the Rogerses, Hall, Whitney, Logan, Hunt, and many others, and grossly unjust to American science and geological history, while full also of groundless personal claims. I review some of these points.

Supposed Triassic of Lake Superior.—Prof. Agassiz admits that he believes Mr. Marcou to be wrong with respect to the Triassic ("New Red") character of the Lake Superior Sandstone, and thus we do not differ as to this one of the claims.

Now this question of the Lake Superior Sandstone is the one that especially calls out Mr. Marcou's opinions of American geologists. Making these rocks, and the Connecticut river and Virginia beds, as well as 500,000 square miles of territory over the Rocky Mountains, "New Red," he is indignant that Hall, Whitney, Logan, Prof. Rogers, etc., do not follow in his track. After giving a one-sided view of opinions on the different rocks which he classes together as *undoubted* "New Red" he says:

"It is difficult to present an age of strata in a manner more ambiguous and *empatée. The brothers Rogers and James Hall try their best to suppress* the New Red Sandstone formation in North America; but they do not know exactly what to do with these five or six thousand feet of strata. On the Geological Map of H. D. Rogers, the New Red Sandstone is unknown in the Magdalen Islands; on the northeast of the Baie des Chaleurs it is colored as Jurassic Red Sandstone, though the *Honorable Sir William E. Logan, Chevalier of the Legion of Honor*, calls it Carboniferous Sandstone. In Prince Edward Island, Connecticut valley, New Jersey, Pennsylvania, Maryland, Virginia and North Carolina, the New Red is colored as older Mesozoic (Jurassic coal and Jurassic red sandstone). In Lake Superior it grows older, and the New Red is colored Cambrian, (Primal, Auroral and Matinal). In the Praries, Texas, Rocky Mountains, New Mexico, etc., the "New Red," that seems to change its age with Protean facility, has once more renewed its youth and is colored as Cretaceous, and sometimes also as umbral and vespertine, or in ordinary language as Lower Carboniferous.

"*They have not thought of putting the New Red in the Upper Silurian or the Tertiary. I would advise these honorable savants to consider if one of these determinations would not be preferable.*"

The *jumble* here is of Mr. Marcou's making, and it comes of his own errors about the "New Red." We let the style of criticism go without remark, satisfied for the present with italicizing only some of the more characteristic parts.

While on this topic, Mr. Marcou, noticing that Dr. D. D. Owen had within a few years taken the same ground with Prof. Hall and other geologists, says, "why Owen changed his views is quite a mystery." He will now regard the case of Dr. Owen not the only mystery.

Permian of the Rocky Mountain Region.—I pointed out in my review that Mr. Marcou had distinguished as Permian, rocks that contained fossils which he set down in his *Field notes* and *Resumé* with a query as a *Belemnite* and a *Pteroceras* (the latter word changed in the recent work to *Gasteropod*), although no Belemnite or Pteroceras is known to occur below the lower Jurassic (Lias). Disregarding or defying the hints from the imperfect fossils, he made the beds Permian on *lithological characters* and superposition alone.

On the Permian of Mr. Marcou, Professor Agassiz says nothing. The use made of lithological characters in its determination is far from sustaining the opinion cited above in paragraph 6.

Triassic of the Rocky Mountains.—My review states that Mr. Marcou established the existence of the Triassic on one fossil, and that an uncertain species of pine wood: this one doubtful fossil wood, and the *lithological characters* make up the evidence in favor of the discovery: and on *lithological characters* and superposition alone he based his queried subdivision of it, into *Bunter*, *Muschelkalk*, and *Keuper*—thus again badly misusing lithological evidence. He mentions also the discovery of a *Cardinia*, but says that Cardiniæ occur in rocks from the Jurassic to the Carboniferous.

Professor Agassiz brings forward nothing against my conclusion that the Triassic was not identified in the Rocky Mountains by Mr. Marcou.

Jurassic rocks in the Rocky Mountains.—The evidence which I cited that Mr. Marcou's Jurassic is really Cretaceous, was based on the determination by Hall, Conrad, Shumard, and others, that his supposed Jurassic fossils are Cretaceous, and that they occur at localities in the west along with known Cretaceous species. Morton's figure of the *Gryphæa Pitcheri* (Morton) I understand was made by Conrad, so that Conrad is certainly good authority as to the identity between it and Mr. Marcou's species. Dr. Newberry, who has recently returned from the Rocky Mountains confirms these conclusions; for he says (see this volume page 33):

"I may say in confirmation of the assertion that your fossil plants [species of Alder, Beach, Credneria, Ettingshausinia, &c.] are Cretaceous, that I found near the base of the yellow sandstone series in New Mexico, considered Jurassic by Mr. Marcou,—a very similar flora to that represented by your specimens, one species at least being identical with yours, associated with *Gryphæa*, *Inoceramus*, and *Ammonites* of lower Cretaceous species."

With such evidence, even the exact identification of the two fossil shells is of little importance. The Cretaceous is the lowest formation in which leaves of any dicotyledons have been found.

Professor Agassiz states that Mr. Marcou is a good Jurassic geologist. But this does not affect the case in hand. For he had but two or three fossils about which to use his Jurassic judgment; and if this judgment has pronounced fossils to be Jurassic that really occur in the west associated with Cretaceous species, or if his knowledge of rocks in Europe has led him to think he can tell Permian, Triassic, or Jurassic rocks by their lithological characters, when he sees them in America, it has served him badly.

We regard it therefore as still true that Mr. Marcou's Triassic of Lake Superior, is not Triassic; and in the Rocky Mountain region, his Permian is not proved to be Permian, his Triassic not Triassic, and his Jurassic not Jurassic. Where are then his discoveries?

Map.—As regards the geological map-making, there is little resemblance between the cases of Rogers and Hall and Mr. Marcou. The former do not claim to be discoverers over the Rocky Mountain region, and Mr. Marcou does. Mr. Marcou, while remarking that the colors to the north and south of the course he followed are only approximative, says, "*I am sure of the limits of the formations on the line I have explored near the 35th parallel of latitude;*" and guided by this *sure* determina-

tion, he marked the Triassic on his map, and then, at a hazard, influenced by his views of earlier explorations, he spread the Triassic color far north over the 500,000 square miles. Now if his identification of the Permian and Triassic was in each case an error, what shall we say of the 500,000 square miles? and what of his map, if this is all wrong, and in addition his identification of Triassic in the Lake Superior region? He cannot rightly shield himself behind any geologist, or the common usage of following the best compiled results for fixing the lines.

Theoretical inferences may be good by way of suggestion; but too eagerly followed they lead to just the errors Mr. Marcou has made. But his system for the West has not even the show of probability in its favor. It is well known, and Mr. Marcou admits it, that Cretaceous fossils and rocks occur about the very summit plains of the Rocky Mountains. The natural inference is, therefore, that when in Cretaceous times these summits were under water, the sea also extended over what are now the eastern slopes of the mountains, and might have covered them with Cretaceous beds: and that thus the Cretaceous should be expected to be the surface formation, (it is understood that the question relates to the *surface* formation, as the colors refer in all cases to this,) and that any Jurassic, Triassic, and Permian, if they exist, should be covered up by it. This, I say, is what should naturally be expected. Moreover, this is what all researches since Mr. Marcou was over the region are tending to prove; they sustain Hall and others in coloring the greater part of the Rocky Mountain slope Cretaceous. The inferior beds, as the Palæontologist quoted from in my paper states, may be looked for as outcropping beds about the base of the ridges or crests of the mountains. Mr. Marcou's map is hence not only at variance with recent researches, but also with reasonable views of western geology.

We cannot see therefore that Mr. Marcou's claims as a discoverer are in any one case sustained, or that his merits are in any respect enhanced by his American researches. And we certainly should not go to him for an exposition of American geology.

Professor Agassiz knows well our American geologists and appreciates their labors; and he writes about them in a different style from Mr. Marcou. But on this point it is not necessary to dwell.

2. *On some points in American Geological History;* by Prof. James M. Safford, of Tennessee.—The Lower Silurian rocks of East Tennessee afford several very interesting local beds. Among them there are two which I desire to mention with reference to their bearing upon American Geological History. The first and oldest is a bed of crinoidal variegated Marble: the second is a bed of sandy *ferruginous* Limestone of peculiar aspect. The Marble (measured not far from Knoxville) is nearly 400 feet thick; the ferruginous bed is thicker and sometimes rests upon the former, but is generally separated from it by a few feet of calcareous shale. The whole is overlaid by limestones and calcareous shales of the Hudson period.

The geographical extent and range of these beds is peculiar, and to my mind, indicates the early Silurian age of the Appalachian oscillations. It is easily shown, that, before they had partaken of the later and Appalachian movements, these beds were long and narrow belts stretching to the

northeast and southwest. The Marble, for instance, ran from Virginia, through Teneessee, to Georgia, a distance of more than one hundred and fifty miles. How much further it extended lengthwise beyond the limits of Tennessee, I know not. Yet with this length its greatest breadth was not more than twenty miles. There can be no question as to the belt-like character of this bed when *first* deposited. Although broken and dislocated by after movements, the original thinning out of the bed laterally is clearly seen in good sections.

The ferruginous bed commenced within the limits of Tennessee, a few miles above the present site of Knoxville, and ran down into Georgia. Its length in Tennessee was not less than 100 miles. How far it extended southwestward into Georgia, I have not ascertained. It covered a somewhat wider area than the marble, but had very much the same long belt-like character.

To what now is the long narrow form of these beds to be attributed? and why did they, or do they, conform in direction to the great Appalachian folds? and why did they thin out on both sides much after the same manner, there being, at this period, no greater indication of dry land on one side than on the other? It appears to me that your view of the Silurian age of the Appalachian oscillations will alone satisfactorily account for these characters. By them the sea-bottom was arranged (perhaps in a long trough) first for the crinoidal grove, the remains of which, together with small corals, (Chaetetes,) form the Marble bed. Afterwards by other oscillations the bottom and the sediment were prepared for the ferruginous bed, &c.

There are other facts bearing upon this subject, which I have observed in the Lower Silurian rocks of East Tennessee, but which time will not permit me to refer to at present.

3. *Post-Pliocene of Lewiston, Maine.*—Mr. W. W. Baker gives an account in the Proceedings of the Boston Soc. Nat. Hist., 1858, p. 394, of the occurrence of a fossil starfish in a hill of earth, 30 miles from the sea, 200 feet above its level, 100 feet above the level of the Androscoggin, which is half a mile off, and 10 feet below the present level of the surface. The hill is clay for eight feet, then thin layers of sand, gravel and clay, alternating. The species according to Mr. Bouvé, was the same as the living species of the coast. There were also numerous impressions of shells.

4. *Untersuchungen über die Entwickelungs-Gesetze der organischen Welt, während der Bildungs-Zeit unserer Erd-Oberfläche*, by Dr. H. G. Bronn: 502 pp. 8vo. Stuttgart, 1858.—This work is a general review of the progress of life in the course of geological history. It received the prize from the French Academy in 1857.

5. *Further Contributions to the Palæontology of the Tilestones or Silurio-Devonian Strata of Scotland;* by Mr. D. Page, (Proc. Brit. Assoc., Ath., No. 1616).—Without entering on the stratigraphical relations of these tilestones (which would be discussed at a subsequent meeting), he might simply mention that part of them, as in Lanarkshire, seemed to cap and form portion of the Upper Silurians, while the larger portion, the Forfarshire flagstones, undoubtedly constituted the basis of the Old Red Sandstone; hence, with a view to avoid all discussion in the mean time, he had ranked the whole as "Silurio-Devonian." Beginning

with the Lanarkshire beds, he had, since the Glasgow meeting, been enabled to add several new forms to the fossil Fauna of that district, for hitherto no trace of vegetation had been detected in the strata. In addition to *Trochus helicites* and *Lingula cornea*, which were then known, he had now to add Pterinea, Orthonota, Nucula, Avicula, Orthoceras, and other well-marked Ludlow or Upper Silurian shells. To the Crustaceans then known, viz., Beyrichia, Ceratiocaris, and Himanthopterus, he had now to add several discoveries which rendered the structure of these curious crustaceans more apparent, besides the detection of two entirely new forms, which he would venture to term provisionally *Eurypterus spinipes* and *E. clavipes*, in allusion to the characteristic form of their swimming paddles, or third pair of organs which spring from the under side of their cephalothorax. Turning to the Forfarshire beds, which in 1855 were known to yield little more than obscure vegetable forms, *Parka decipiens* of Lyell, Pterygotus, and Cephalaspis, he was now enabled to add several new and gigantic forms of Fucoids, a Cyclopteris, and a Lepidendroid stem, which was clearly of terrestial origin. To the Fauna he has added gigantic Scolites or annelid burrows, Serpulites or annelid tracks, and an organism which appeared to be the remains of an annelid itself. There had also been discovered several new portions of Pterygotus, which rendered the true structure of that gigantic crustacean much more apparent; and he had also been enabled to describe and figure two new crustaceans under the names of Kampecaris and Stylonurus, the latter closely related in structure to Eurypterus, and approaching the forms of those found in the Lanarkshire strata. To Cephalaspis, of which little more was known than the head and bony ring-plates of the body, he had now to add a well-marked corneous eye capsule, a pair of pectoral fins, a dorsal fin, and the true form of the large heterocercal tail,—so that, instead of figuring this much-caricatured fish as had hitherto been the case, as a saddler's knife for the head, and a parsnip with a few radicles for the body, we could now restore it as a legitimate and elegant fish, much resembling in general contour the armed bullhead or Aspidophorus of our present shores. There had also been discovered a vast number of fin-spines or Ichthyodorulites, which were yet undescribed, and a small fish with fin-spines and shagreen-like scales, to which he had given the name of *Ictinocephalus granulatus*, in allusion to its kite-shaped head and shagreen-covered body. For the discovery and preservation of these new fossil forms, palæontologists were mainly indebted to James Powrie, Esq., of Reswallie, Forfar, and to Mr. Slimon, surgeon, Lesmahagow, to the latter of whom the British Association, on the representation of Mr. Page, has given a grant of 20*l*., to assist in prosecuting his researches among these interesting but as yet partially explored strata.

III. BOTANY AND ZOOLOGY.

1. *Nereis Boreali-Americana; or Contributions to the History of the Marine Algæ of North America;* by William Henry Harvey, M.D., M.R.I.A., F.L.S., Professor of Botany in the University of Dublin, etc. Part III. *Chlorospermeæ*. (Smithsonian Contributions, for 1858). Pp. 140, tab. 37–50, imp. 4to.—Our readers are familiar with the first part of this elaborate work, containing the *Melanospermeæ* or olive-colored

Algæ, and with the second, the *Rhodospermeæ*, or rose-red series. With the *Chlorospermeæ*, or proper green Algæ, Dr. Harvey has now completed this extensive undertaking, and furnished us with a manual of the highest character, by which the marine Algæ of our continent,—and also a good part of the fresh-water species—may be readily studied. Great praise and cordial thanks are due to Dr. Harvey for the prolonged and assiduous labors which have fairly opened this wide and difficult field to the American student; and likewise to the Smithsonian Institution, for its enlightened liberality in the publication, not only presenting copies to all the principal public libraries of the world, but placing a separate edition upon sale at a low price, which brings the work within the reach of every earnest student or zealous amateur in the country. We are gratified to learn that the enterprise and good judgment of the managers of the Institution are duly appreciated by the public; and that the work has achieved a popularity unsurpassed by any of the valuable Smithsonian Contributions to Knowledge.

No good account of the *Chlorospermeæ* could be given without some view of the fresh-water species also. So, as many of these as was possible under the circumstances, have been included in the present memoir. But a large part of them can be investigated only when alive. Also the *Diatomaceæ* and *Desmidiaceæ*, although they systematically belong to the *Algæ*, yet they form a microscopic world of themselves, and require a separate treatment by a special monographer. The accomplished naturalist upon whom this task appeared to devolve, and who, indeed, had done much towards its accomplishment, is no more; he of whom our author feelingly writes, in the following extract.

"I must therefore leave the task of [more fully] describing the fresh-water *Algæ* of America to other hands;—to some one living among them; and having eyes fully open to the difficulties of his task, and zeal and ability to work it faithfully. And here I cannot omit a slight tribute to the memory of one in whom were combined, in no common degree, the qualifications which make an able naturalist, and who, had he lived, would probably have taken up the broken thread. I allude to the late Professor J. W. Bailey, of West Point, one of the earliest explorers of American *Algæ*, and whose very able memoirs on the *Diatomaceæ* have won for him an imperishable name in the annals of science. To me his loss is more personal than to most of his botanical friends; for, from the hour we first met there grew up between us a warm friendship, which death has interrupted, but which I trust it has not ended. He it was who first suggested to me a memoir on the American *Algæ;* he arranged with the Smithsonian Institution the terms of its publication; he supplied me with a multitude of specimens; and to his influence I owe the assistance I have received from many American algologists, who looked up to him for direction in their studies. He was, as far as the Algæ are concerned, my chief American referee, to whom I could apply when seeking information on local matters connected with this branch of study. With him I constantly associated my work, and to his approbation I looked forward as the most grateful reward of my labors; and now that he is removed, my interest in the work has sensibly flagged, and I am not sorry that it is brought to a conclusion."

A passing tribute is paid to the memory of another contributor, the late Professor Tuomey of Alabama; and his name is commemorated in one of the two new genera described in the present part. *Tuomeya fluviatilis* is a fresh water plant, resembling a *Lemanea* in external appearance, but very different and not a little curious in anatomical structure.

The other new genus, *Blodgettia*, is named for the late Dr. Blodgett of Key West, who had zealously collected the Algæ as well as the other plants of the Florida Keys.

The structure of *Blodgettia confervoides* (which is illustrated in one of the plates) is so extraordinary and unexpected, that we are tempted to copy the account of it. Briefly, the cell-wall of its unicellular joints is "formed of separable membranes, the outer of which are hyaline and homogeneous, the innermost traversed by parallel, longitudinal, anastomosing veinlets. Spores seriated in moniliform strings, developed from the veinlets of the inner cell-wall!" The following is the detailed account.

"The highly curious little Alga on which the present genus is founded so closely resembles a Cladophora that it will readily pass for one, unless it be very closely examined under a powerful microscope. Indeed so great is the resemblance to a branched Conferva that I formerly distributed it to my friends with the manuscript name of Cladophora cæspitosa, under which it was my intention to have described it in the present work; nor did I discover my error until I commenced making sketches for the plate now given. I was then first struck by the peculiar opacity of the dissepiments; and afterwards by what looked like a compound cellular structure in the walls of the cells.

On applying a higher power, other characters came out which induced me to dissect one of the articulations, when I discovered the curious structure of the inner membrane or primordial utricle; in which (as far as I can make out) the spores are developed. To see the structure as above described, the readiest mode is to proceed as follows. Cut off a portion of one of the long cells which terminate the branches; place it on the table of a dissecting microscope, moisten it, and you may readily express the viscid endochrome, which generally contains, besides the usual starch and chlorophyll grains, a number of pyramidal crystals; but these are probably adventitious. When the endochrome has been pressed out, the structure of the inner cell-wall may be partially seen; but to see it clearly, the outer coats must be removed. This may readily be done, either by tearing, with a pair of dissecting needles, or by making a longitudinal section through the cell, when the different coats easily separate, on the section being *teased* in a drop of water. The outer coat, coats (for there are two or more, though the secondary ones sometimes elude detection, owing to their extreme tenuity) are quite transparent and structureless, as is usually the case in the walls of cellular tissue. But the inner coat offers a peculiarity of structure which I have not noticed in any other Algæ, nor have heard of its occurrence in the cells of any other plants. At first sight the membrane seems to be composed of numerous minute, elongated fusiform cellules, not unlike the wood-cells of phanerogamous plants, but totally unlike any algæ cells known to me. Careful examination has however convinced me that the appearance of cellular

structure is deceptive; and that the membrane itself is homogeneous, but traversed by slender filiments or nerves, which anastomose together, forming areolæ which look like cells. These filiments give off *free* ramuli whose apices swell into spores; and (probably) by repeated cell division produce the strings of roundish *spores*, which are so conspicuous in most of the areolæ. The appearance of the whole membrane with its spores is as if a number of the *asci* of a lichen were placed side by side; the true structure, however, I need hardly say, is widely different."

Truly nature revels in variety, in the lowest and simplest, even more than in the highest tribes of plants. *Hydrodictyon*, or the Water-net, affords another, and a more familiar illustration of this, being a viviparous Alga. While in a *Conferva* a zoospore develops into an individual which increases in dimensions by the multiplication of its cells, in *Hydrodictyon* a great number of zoospores combine to form one individual, composed of a definite number of cells which remain unchanged, until each cell gives birth to a new *Hydrodictyon* complete.

"In all stages the *Hydrodictyon* is a bag-like or purse-shaped net, with generally five-sided meshes,—each mesh consisting of a single articulation or cylindrical cell, united by its ends to the neighboring cells, ... and from first to last carrying on an independent existence. When first emitted from the parent, the young *Hydrodictyon* is of microscopic size. It grows rapidly until each articulation becomes from a quarter to half an inch in length, and half a line in diameter. Up to this period the cells are filled with a green semi-fluid endochrome, in which grains of different sizes are formed. Gradually this green matter is resolved into an infinite number of minute zoospores, which are at first spherical, afterwards ovate, pointed at one end, and which, while contained within the cell-wall, exhibit lively movements. At length these movements gradually subside, and the zoospores arrange themselves, end to end, into polygonal, commonly pentagonal areolæ; and when all the zoospores contained within a single articulation have so arranged themselves, the little net is completed before its emission or birth. When all is thus ready the parent net falls to pieces, each articulation floating separately; and shortly afterwards, on the bursting or deliquescence of the wall of the mother-cell, the little net work floats independently, and commences its career of growth and development."

Here only the spores or germs are active; the developed plant vegetates quietly. But in the Oscillatorias, which are described a little further on, the mature plant exhibits very *animated* movements. "Some have a rapid progressive and regressive movement, by which they can change their place, rising or falling in the water; others, while remaining nearly in one place, move from side to side, describing an arc. The genus *Oscillatoria* is so named from the pendulum-like movements of its filaments. Species of this genus are to be found in most pools of stagnant water, and their peculiar movements may be easily observed. These plants occur, when fully developed, in floating, skin-like, slimy pellicles, of a deep green, or blackish, or bluish color, and a gelatinous substance. If a small portion of the floating scum be placed in a cup of water, and allowed to remain for some hours at rest, its edges will become finely fringed with delicate, radiating threads, which extend further and further

from hour to hour..... The filaments have merely spread out, not grown, by means of their peculiar movements. These movements are of three kinds; first, there is the oscillating movement, one end of the thread remaining nearly at rest, while the other sways from side to side, sometimes describing nearly a quarter of a circle in a single swing. Secondly, the tip of the filament has a minute movement, bending from side to side, like the head of a worm; and thirdly, there is an onward movement, probably the result of the two former. It is this latter which causes the filaments to radiate, and spread out from the edges of the stratum."

But we must refer the curious reader to the work itself, which, though framed on a systematic and strictly scientific basis, is yet replete with physiological matters of general interest. We could have wished for a detailed account of the recent discoveries, by Pringsheim and others, of the sexual fertilization of the spores of the lower Algæ, which takes place in a variety of wonderful ways. Reference, however, is made to the original memoirs, of which, indeed, some abstracts have been given in former volumes of this Journal.

The plates, although, from the nature of the subjects, not as showy as those of the second part, are handsome and admirable. A. G.

2. *Species Filicum; being descriptions of all known Ferns. Illustrated with plates;* by Sir William Jackson Hooker, K. H., &c. London: 1858. William Pamplin. Parts VII and VIII, or Vol. II, Parts III and IV. 8vo, pp. 250, tab. 111–140.—The publication of this work is resumed after an interval of six years. This delay, we are told, "has been a source of great regret both to the author and to the publisher, and has been, in a measure, occasioned by the great difficulty and labor of the subject in hand,—especially in a genus so extensive as that which occupies nearly the whole of the portion now issued, viz., *Pteris.*" Besides *Pteris* the author has here elaborated *Llavea*, *Cryptogramme*, *Pellæa* and *Ceratopteris*. *Llavea* consists of a single Mexican species separated from *Pellæa* by habit rather than by any characters of fructification. *Cryptogramme* is also considered as monotypic, *C. crispus* being made to include the East Indian *C. Brunoniana*, and the North American *C. acrostichoides*. The older name of *Allosorus* is rejected both for this genus and for *Pellæa* on account of the vagueness of its character, as originally drawn up by Bernhardi; '*Sporangia cathetogyrata, sessilia, sub-aggregata. Hyposporangia sub-communia, margine libero, sub-pellucida;*' and because it "has been made a receptacle for Ferns of very varied structure, according to the different views of authors respecting the limits of the genera, especially of those included in this work under the name of *Pellæa.*" The genus *Pellæa* was proposed by Link in 1841, adopted by Fée, in his Genera Filicum, and here accepted by Sir William Hooker. It includes thirty-three species, of which one third are found in the United States, viz., *P. gracilis* in the Northern States; *P. atropurpurea* from Vermont to Texas; *P. Bridgesii* and *P. densa* in California; and *P. cordata*, *P. flexuosa*, *P. andromedæfolia*, *P. pulchella*, *P. Wrightii*, *P. longimucronata* and *P. Orithnopus* in the region between Texas and California. The last three species are probably but forms of one, which must bear the name of *P. mucronata.* [*Vide Botany of Mexican Boun-*

dary Survey.] Under the genus *Pteris* the author remarks: "It will be seen from the characters and the references given, that I have taken the bold step, and what many will consider a retrograde movement in botany, in restoring almost entire the original Pteris of Linnæus and Swartz and Willdenow." Reviewing at some length the various modern genera, *Campteria*, *Litobrochia*, *Doryopteris*, *Heterophlebium*, *etc.*, into which various authors have divided the Linnæan *Pteris*, he concludes with the opinion that "as new light is continually being thrown upon this family of plants, it is premature to sanction the great multiplication of genera by laying stress on the nature of the venation when unaccompanied by any corresponding changes in fructification, or any marked differences in habit, and more philosophical to consider such groups in the light of sections or subgenera. The importance of the vascular structure is acknowledged; an arrangement, to say the least, equally natural, is preserved, and some degree of stability is given to names invented and sanctioned by the most illustrious botanists that ever lived." This mode of regarding *Pteris* is perfectly in accordance with the way in which *Lindsæa* and *Adiantum* were treated in the earlier part of the work, and on the whole must recommend itself to the student of general descriptive botany. The investigator of a single natural order is peculiarly liable to commit the fault of excessive subdivision, while a botanist, like Sir William Hooker, eminent in all departments of his science, will naturally take wider, not to say sounder, views of the limits of genera and species. One hundred and twenty-three species of Pteris are admitted, not including a large number of obscure species, of which no notice is taken, and the fifteen or twenty nominal species, which are reduced to the cosmopolitan *Pteris aquilina*. *Ceratopteris thalictroides*, Brongn., after having been referred to five different suborders by various botanists, and by one even excluded from the family altogether, is here associated with *Pterideæ*, not from any real similarity of structure and habit, but because no better place has been found for it. We may remark in passing, that it is not quite correct to say, as on page 155, that Agardh excluded only the *Allosori* of Bernhardi and Presl from the Linnæan *Pteris*, since in fact, he excluded all those that Swartz called "*Adiantoideæ*;" among them, *P. pedata*, the type of Mr. J. Smith's genus, *Doryopteris*. *Pteris dispar*, Kunze, in *Bot. Zeit.* 6, p. 539, is evidently the same plant as *P. semipinnata*, var. β, and should have been given as a synonyme of it. An advertisement affords the gratifying intelligence that the first part of the third volume may be expected at an early day. D. C. E.

3. *Catalogue of North American Birds*, chiefly in the Museum of the Smithsonian Institution, by SPENCER F. BAIRD, Assistant Secretary of the Smithsonian Institution.—This Catalogue by Prof. Baird, enumerates 738 species of birds, of which all but 22 are strictly North American. All the 738 were determined from specimens, excepting 31. Wilson's work in 1814 contained 283 species, Bonaparte's in 1838, 471 species, and Audubon's in 1844, 506 species. The catalogue therefore contains nearly one-half more species than Audubon enumerated. The Catalogue is especially valuable as a list of well ascertained localities, thereby indicating the range of the species. The author's habitual care and untiring zeal in research authorize full reliance on its accuracy.

4. *Odontology*, by Prof. OWEN.—A very extended article of great value on Teeth, very fully illustrated, is published by Prof. Owen, of London, in the 16th volume of the Encyclopedia Britannica (8th edition). It takes up the whole range of vertebrates and includes both fossil and recent species among its illustrations. The wood-cuts, of which there are near 200, are excellent and highly instructive.

IV. ASTRONOMY.

Donati's Comet, or the Great Comet of 1858.—In our last number we were favored by Prof. Wm. C. Bond, of the Observatory of Harvard College, with a brief notice of this splendid Comet. In Runkle's *Mathematical Monthly*, Nos. 2 and 3, (Cambridge, Mass. Nov. and Dec. 1858,) Mr. Geo. P. Bond, of the same Observatory, has published a highly interesting and valuable account of this body. Rarely has so fine an opportunity been presented for observing the physical constitution and internal changes of a comet, and the opportunity has been well improved. The second part of Mr. Bond's paper occupies 26 pages and is enriched by numerous wood-cuts, and by two engraved telescopic views of extraordinary beauty. We cite a few passages from this important memoir, and hope that our readers will procure and read the full account.

"The interest of the telescopic view, taking all the circumstances into account, the size of the instrument, the perfect purity of the atmosphere, and the splendor of the object, have rarely been surpassed. The nucleus and the outline of its nearest envelope were visible in full sunshine with the large telescope. The head of the Comet could be seen with the naked eye at twenty minutes after sunset, at which time the second envelope was discernible with the telescope. It is most remarkable, that, with all this accession of brightness, the nucleus itself had now diminished to a diameter of only four or five hundred miles, scarcely one fifth of what it was on the morning of the 9th of September, by a very careful determination. Its volume had thus diminished to *one-twentieth* part only. The remaining nineteen-twentieths had, in the intervening period, expanded into the tail, or had gone to form the envelopes which now encircled it, by a process which has been fully illustrated in the preceding pages. But are we then to conclude that the nucleus, in the focus of these mysterious operations, had in this way expended the greater part of its substance? To this inquiry the best reply is a consideration of its subsequent condition. After several more eruptions from its surface, similar to those above described, it receded from our view about the 20th of October, with an evident *increase* of size compared with its condition two weeks before, and still shining with its accustomed intensity."

"Examined in the day time on the 5th with the highest powers which it would bear, no indication of a *phase* could be seen. * * "

"The tenth of October was the day of nearest approach to the earth, but the comet was manifestly on the wane, though expanded over a larger extent of the sky than before. Five envelopes, reckoning the exterior haze as one, could be traced through the whole or some part of their outline."

"We must add a few words on the appearance presented by the tail between the 6th and the 10th of October. At the date first named, one

of the supplementary rays, attained a distance of 55° or fifty millions of miles from the nucleus, somewhat exceeding that of the principal tail, and in a direction as usual, nearly in a line from the sun. Others less perfectly developed could be discerned near a point where the curvature of the main stream was pretty suddenly changed. On the 8th five or six transverse bands could be distinguished in the tail half a degree or less in breadth, with clear, well-defined outlines, and perfectly resembling auroral streamers, excepting that they kept their position permanently, that is, without motion sensible to the eye, they diverged from a point between the sun and the nucleus."

"The train attained its largest apparent dimensions on the 10th, when the main stream of light could be distinguished through an arc of 60°, corresponding to a length of fifty-one millions of miles, or rather more than half the distance of our earth from the sun. The distribution of its light at a distance of 20° or 30° from the nucleus in parallel or slightly diverging bands, alternating with dark spaces, was strongly exhibited. They were 5° long, and 20′ or 30′ wide, and might aptly be compared either to the streamers which often break up the continuity of an auroral arch, or to a collection of five or six tails of small comets, forming from the remains of the large one."

"The most recent intelligence leaves no room to doubt that the comet of Donati is periodical, having a time of revolution of about two thousand years. The following are the results arrived at by different computers:—

Watson,	2415 years.
Bruhns,	2102 "
Löwy,	2495 "
Graham,	1620 "
Brünnow,	2470 "
Newcomb,	1854 "

"Supposing its last perihelion passage to have occurred at the beginning of the Christian era, it must have passed its aphelion in the early part of the tenth century, at a distance of 14300 millions of miles from the sun, its velocity at that point being 480 miles an hour."

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Tables, Meteorological and Physical, prepared for the Smithsonian Institution;* by Arnold Guyot, LL.D. 2d edition, revised and enlarged. Washington, Smithsonian Inst., 1858, 8vo.—The full value of this extensive and important collection of tables it is impossible to exhibit without a detailed catalogue, which our limits do not permit. They are selected from a wide range of authorities, by a gentleman who is well known for his thorough acquaintance with the best sources of knowledge on these subjects, and for his faithful and exact computations. The amount of labor demanded for the preparation of the work must have been great, and the scientific men of all countries are under weighty obligations to the accomplished editor, and to the Secretary of the Smithsonian Institution for making accessible at so cheap a rate, so precious a collection of auxiliary tables.

A first edition of part of these tables appeared in 1852. More than three times as much matter is contained in the present volume, as in the former issue.

The work now comprises six series of tables, viz.:

I.	Thermometrical,	15 tables,	in 35	pages.
II.	Hygrometrical,	33 "	" 165	"
III.	Barometrical,	28 "	" 134	"
IV.	Hypsometrical,	26 and 44 "	" 149	"
V.	Meteorological Corrections,	99 tables	" 120	"
VI.	Miscellaneous,	6 "	" 12	"

The following remarks are made by Prof. Guyot in respect to the construction of these tables:

"In the Thermometrical series six small tables have been added; they were prepared for converting into each other differential results given in degrees of any one of the three thermometrical scales, irrespective of their zero point.

"The Hygrometrical series has been entirely reorganized. It only contained five tables, all in French measures, and the appendix. It is now composed of twenty-seven, arranged in three divisions. In the first are found ten tables, based on Regnault's hygrometrical constants, both in the psychrometer, the dew-point instruments, and for computing the weight of vapor in the air. The whole set in English measures, and Table V in French measures, have been prepared for this edition. Being based on the best elements we now possess, they are given here for ordinary use. The second division contains the seven most important tables published in the *Greenwich observations*, and Glaisher's extensive Psychrometrical Table. These tables being much used in England, and the results obtained by them exhibiting no inconsiderable differences from those derived from the preceding ones, they are indispensable for comparing the results. The third division, composed of ten miscellaneous tables, furnishes the means of comparing the different values of the force and the weight of vapor, especially those which have frequently been used in Germany, and also of reducing the indications of Saussure's Hair Hygrometer, to the ordinary scale of moisture. The appendix remains as in the first edition, but all the tables have been revised and corrected.

"The barometrical series now in four divisions, has been increased from twelve to twenty-eight tables. Excepting three small tables for capillary action, all the new ones have been computed for this edition. The comparison, now so much needed, of the Russian barometer with the other scales, appears here for the first time.

"The Hypsometrical series is almost entirely new. It contained only Delcros's table for barometric and Regnault's table for thermometric measurements, besides two auxiliary tables and the thirteen small tables of the appendix. It now offers twenty-three tables for barometrical measurement of heights, in which all the principal formulas and scales are represented; three for the measurement of heights by the thermometer, in French and in English measures; and a rich appendix of forty-four tables, more extensive and convenient than those in the old set, which afford the means of readily converting into each other all the measures usually employed for indicating altitudes.

"The series of Meteorological corrections for periodic and non-periodic variations, for all parts of the world, mostly due to the untiring industry of Professor Dove, is an addition which will surely be appreciated by those who know how difficult access to the original tables is for most meteorologists. A few tables have been added to Dove's collection, computed by Glaisher, Captain Lefroy, and by myself. Most of the tables refer to temperature, only two to moisture. Two tables of Barometrical corrections have been placed in the Hypsometrical series, where they were needed, until they can be joined by others to make a set in this series, which still awaits new contributions, especially for these two last departments."

2. *On the Heating of the Atmosphere by Contact with the Earth's Surface;* by Prof. HENNESSY, (Proc. Brit. Assoc.. Ath., No. 1616).—The temperature of the atmosphere depends principally on the heat which it receives from the sun and on what it loses by radiation. A portion of the solar heat is absorbed in passing through the air, while another portion penetrates to the earth's surface. The ground becomes thus heated, and the lower strata of the atmosphere acquire the greater part of their heat from contact with the warmed surface. It is admitted that the mode in which the air becomes heated by contact with the ground must be a kind of circulation analogous to that seen in the movements of a heated mass of liquid, such as boiling water. When studying the vertical movements of the atmosphere, with reference to which Prof. Hennessy had made a communication to the Association last year, he had been led to consider the connexion between such movements and the influence of the heated ground. In order to experimentally study the question, thermometers were suspended at different heights above the ground, and under different circumstances of exposure to the influence of the supposed currents. Observations were made every minute, and sometimes every half minute, during short intervals, about the middle of the month of May, on days when the sky was clear, and during which there was consequently a great deal of solar radiation. In general the thermometers exhibited fluctuations of temperature, the intensity of which diminished the more they were protected from the influence of circulating currents in the air. The greatest fluctuations were presented by thermometers with blackened bulbs exposed in the sun. This arose from the circumstance that the blackened bulbs, by acquiring a high temperature, became themselves disturbing agents in the calorific conditions of the surrounding air. Evidence of similar phenomena appears to be presented by the curves of temperature obtained by the aid of photographical registration at the Radcliff Observatory in Oxford. Attention has been called by Mr. Johnson to a remarkable serration in the temperature curves during the day. This serration is found only when there is a considerable amount of solar radiation, it disappears during sunless and cloudy weather. While it is explained by referring it to the influence of the solar heat upon the ground, and the consequent circulation of small atmospheric currents, it affords a very satisfactory confirmation of the trustworthiness of the photographical method of registration.

3. *On the Decrease of Temperature over Elevated Ground;* by Prof. HENNESSY, (Proc. Brit. Assoc., Ath., No. 1616).—He showed that the

decrease of temperature in ascending through the atmosphere depended not only on height above the sea level, but also upon the absolute height above the nearest surface of solid land. In this way the decrease of temperature over plains, mountains, and plateaus, would be necessarily very different, and we cannot immediately infer the state of the phenomena in the two latter instances from what may exist in the former. Some of the results of observations made on some of the hills and mountains of Ireland during the Ordnance Survey, as contained in the volume recently published by Col. James, were referred to as illustrations of these general views.

Admiral FitzRoy thought that one circumstance was too much overlooked by Prof. Hennessy in these researches, namely, that along with these ascending currents the whole body of the air was carried along by horizontal currents, so that it could not be assumed that it was the very same air which gave some of the indications which afforded the others. Again, it had been clearly shown that a thermometer placed upon the ground, or close to it, frequently fell 17° or 18° below one placed a few feet or inches above it, while somewhat higher up still, the indications of the thermometer again fell, thus clearly indicating a spot at which there was a maximum temperature. As to the latter part of what he stated, it was so commonly observed that if you placed a thermometer in the lower window of a house, and another in the window immediately above it, in nine cases out of ten you would find the latter indicate a lower temperature than the former. Prof. Stevelly said that, besides what Admiral FitzRoy had pointed out, there were two other circumstances of much importance to be attended to in such observations as Prof. Hennessy had been making. First, that evaporation was going on more or less rapidly according to the circumstances of the locality where the observations were conducted. Secondly, that the air, when having,—either gradually, as in some cases, or abruptly, as in others,—to ascend in its course very elevated ground, was compelled to contract in volume, become condensed, and yet in some cases part with a portion of its vapor, and thus form the cloud which we so often saw capping the hills, as well as giving origin to the high winds and storms which so frequently prevailed there. Dr. Tyndall said that he had just returned from Switzerland, where, on the tops of Monte Rosa, and even of Mont Blanc, he had a full opportunity of witnessing these phenomena on a scale of grandeur truly sublime. The snow in these regions was naturally as dry as dust, and he had frequently an opportunity of witnessing columns of it whirled up to an immense height by the ascending currents of air, into regions where it was soon dissipated, or melted and dispersed into vapor. It was also to be observed that the sun's heat had a power of penetrating water and other screens, such as the clouds formed, far surpassing that possessed by heat derived from less intensely ignited or heated sources, as for instance, from bodies heated red hot, or from vessels filled with hot water and the like. Hence, the sun's rays, though they penetrated the clouds and the earth, yet there they totally lost their former powers, and when radiated back possessed no such power as before of penetrating clouds or other screens, and thus the earth and its atmosphere became a kind of trap for the solar rays.

4. *Death of Gen. Sir William Reid, R. E.*—Late advices from England bring us the sad intelligence of the death of this eminent man, which took place at his residence in London on the 31st of October. His departure is a loss, not merely to the country in whose service his life was spent, but to the world. His modest, unobtrusive worth, his unfaltering courage, his untiring industry, his disinterested efforts to promote the welfare of his fellow-men, and his valuable contributions to our knowledge of the winds, all demand more than a passing notice of his death.

Sir William Reid was born at Kinglassie in Fifeshire, Scotland, in 1791, and was the eldest son of the Rev. James Reid, a clergyman of the Scotch Church. He was educated at the Military Academy at Woolwich, England, and entered the British army in 1809 as an engineer, and served during the last four years of the Peninsula war, under the Duke of Wellington. He was actively engaged at the seige of Ciudad Rodrigo and St. Sebastian's. In the sanguinary assault upon the latter fortress, he headed one of the storming parties, was wounded by a musket ball, and fell, covered with blood, which streamed from his mouth and nostrils. He was supposed to be dead, but, on removing from his neck a black silk handkerchief (which, by advice of a medical friend, he had unwillingly assumed, instead of the stiff military stock), it was found pressed into the wound; and on using a little force to withdraw it, the ball came out with it, not a thread of the handkerchief having been severed. The removal of the handkerchief revived him, but the surgeons, on examination pronounced the wound mortal. Contrary to their expectations, he recovered. He was wounded four times during this war, and had three horses shot under him.

After the conclusion of the peace with France, he served on the coast of America, under Gen. Lambert, until the conclusion of the war here, when he rejoined the Duke of Wellington in Belgium, in 1815. In 1816 he served in the expedition against Algiers; was Adjutant of the corps of sappers for some years after the peace. Afterward, as a Major of engineers, he was employed in restoring the government buildings ruined in the destructive hurricane which devastated Barbadoes in 1831. While so employed, curiosity led him "to search everywhere for accounts of previous hurricanes, in the hope of learning something of their causes and mode of action, but in the West Indian histories he could find little beyond details of the losses in lives and property, and no attempt to furnish data whereby the true character or the actual courses of these storms might be investigated." While thus engaged, the first of the meteorological papers of our lamented countryman, Wm. C. Redfield, met his eye, being that published in the American Journal of Science in 1831. This he says, "was the first paper he had met with which appeared to convey any just opinion on the subject of hurricanes," and, strongly impressed that Mr. Redfield's views were correct, he determined to verify them "by carefully charting the observations made at different points." He soon became satisfied of the rotative character and determinate progress of the gales and hurricanes of the North Atlantic, as maintained by Mr. Redfield. In 1838, having been able to devote more attention to these inquiries, he published his first paper on hurricanes in the second volume of "Professional Papers of the Royal Engineers," and soon after having prepared

himself by a careful analysis of various hurricanes, he published his valuable work entitled "An attempt to Develope the Law of Storms by means of Facts, arranged according to place and time." Of this, three editions have appeared. Some years later (1849) he published a second work entitled "The Progress of the Development of the Law of Storms and of the Variable Winds, with the practical application of the subject to Navigation." By these labors, with those of Redfield, Piddington and Thorn, his principal co-workers, the power of knowledge has conquered even the hurricane, and the intelligent mariner, warned by indications of the barometer, and those of the early winds of the coming storm, may securely watch its approach, and avoid, in almost all cases, its dangerous vortex, and thus sail on unharmed by the gale—even while skillfully using its outer winds to expedite his voyage.

But his meteorological labors, valuable as they have been, formed but a portion of his usefulness. In 1838 he received, unsolicited, the appointment of Governor of Bermuda. On his arrival there in 1839 he found agriculture far behind; corn and hay were imported; there was but little fruit; bitter citron trees grew everywhere; and in sight of the government house was a wide swamp. Col. Reid set the example of improvement. He grafted a sweet orange on a bitter citron tree, in front of the government house; it bore good fruit, and soon all the bitter trees were grafted. He drained the swamp, imported plows and other improved agricultural implements from New York, had plowing taught, gave prizes for the best productions, and in 1846 held a grand agricultural *fete* in a fine dry meadow field—the old swamp. In fact, he gave new spirit to the people, showed them how to work out their own prosperity, changed the face of the island, took great interest in promoting popular education, in diffusing *temperance* tracts, and so won the title of "*the good Governor*," by which he is still affectionately remembered in Bermuda. In one of the volumes of Dickens' Houseword Words, the praises of this "Model Governor" may be found set forth.

In 1846 he was transferred to the Windward West India Islands, comprising Barbadoes, St. Lucia, St. Vincent, Grenada, Tobago and Trinidad. Here also, by his firm and beneficent conduct, he gained the confidence and good will of the entire population, and devoted himself, as he had done at Bermuda, to the welfare of his people and to their advancement in agriculture, education and temperance. He removed a judge who had used his power to oppress the people, and when the home government hesitated to give him their support in this, he promptly resigned his office, and returned to England in 1848. In 1849 he was appointed Commanding Engineer at Woolwich, and commanded the engineer officers and sappers and miners at the Great Industrial Exhibition; and on the resignation of Mr. Robert Stephenson, Col. Reid was requested by the Royal Commission to become, in his room, Chairman of the Executive Committee, in which capacity he served with unremitting attention. On the closing of his service for the Great Exhibition—for which he generously declined remuneration—he received the Order of Knighthood from the Queen, and in September, 1851, he received the unsought appointment of Governor of Malta—no idle appointment, for presages of the Russian struggle were even then flashing in the eastern sky, and the

government knew the value of the man they assigned to this most responsible post. During the eventful struggle which ensued on the banks of the Danube and in the Crimea, Malta was the chief point of embarkation of British troops. In February, 1854, Gov. Reid writes to a friend in this country, "I am preparing for the Russian *storm*—the first portion of 10,000 men from England having just now entered the harbor. I must in charity believe the Czar to be mad, thus to compel mankind to begin anew to destroy each other." He continued in this post until after the close of the Russian war, receiving meanwhile the promotion to General. Near the close of 1857, Lady Reid's health, which had suffered severely from the debilitating climates of the West Indies and Malta, forced her to return to England, and Gen. Reid, resigning his government, followed her as soon as a succession could arrive. Lady Reid lived but a short time after her return, and her husband has survived her but a few months. Gen. Reid had always greatly desired to make a friendly visit to the United States, but was never permitted that pleasure. Lady Reid, with two of her daughters, spent the summer of 1845 in this country, and those who had the pleasure of meeting her will not soon forget the charm of her vivacious and intelligent conversation. Gen. Reid has left no sons, but five daughters, two or three of whom are married to officers in the military and naval service of Great Britain.

We can hardly avoid noting here the remarkable fact that the spirit of this statesman and philosopher who had done so much to illustrate the path of the winds, should pass away, almost, as it were, on the wings of one of the most extensive, rapidly progressing and destructive ocean hurricanes on record. First coming under notice at the Windward Islands about the 20th of October, it passed over Porto Rico, Hayti and the Bahamas; then recurving, its axis passed, on the 24th, nearly over Bermuda, where its violence was extraordinary; and thence for some days following, it pursued its course to the northeastward, almost or quite to the shores of Europe. No storm described by either Redfield or Reid seems to have had the enormous diameter of this. It was severely felt 700 miles eastward of Bermuda in the same latitude, while its western border grazed New York, affecting the barometer sensibly, and rolling in upon us the extraordinary tides of October 24 and 25.

5. *Journal of the Royal Dublin Society.* Vol. I. 441 pp. 8vo. Dublin, 1858.—This first volume of the Journal of the Royal Dublin Society contains a variety of valuable papers—among which, there are Captain F. L. M'Clintock's Reminescences of Arctic Travel, with a geological map and descriptions of fossils, by Rev. S. Haughton; several papers on British, Australian and South Pacific Crustacea, by Dr. Kinahan; Observations on the Climate and Zoology of the Crimea, by Mr. William Carte.

6. *Chimie appliqueé à la Viticulture et à' l'Œnolagie*, Leçons professées en 1856, par M. C. Ladrey, Prof. de Chimie à la Faculté des Sciences de Dijon, etc. 640 pp. 16mo.—This is an important volume to all cultivators of the grape as well as to those who manufacture wine from the grape. Professor Ladrey is an able chemist, and has prepared a thorough work. It treats of the growth of the vine—chemical composition of the ashes—nature of the soil—wine growing countries—fertilizers—organic matters of the vine—of grapes, their kinds, etc.—fermentation, etc.—diseases of the vine, etc.

7. *Post-Pleiocene Fossils of South Carolina;* by FRANCIS S. HOLMES, A.M. Nos. 1 to 5, in 4to, with two lithographic plates to each number. Price $2.00 per number.—This new work by Mr. Holmes, is published in the same style as the *Pleiocene Fossils*, and with similar beautiful illustrations. Figures of about 100 species are given in the 5 parts issued. The volume will be completed in 15 parts, 5 parts appearing annually. From Mr. Holmes we learn that the Post-pleiocene beds extend along the entire seaboard of the state of South Carolina, outcropping about 10 miles from the coast. At the Artesian well, in Charleston, the auger penetrated the stratum of blue mud containing Post-pleiocene shells at the depth of 21½ feet. There was first 2 feet of mud; then 2 feet or more of laminated blue sands and clays; then blue mud again, and so on for 49 feet 5 inches from the surface, when it reached the Eocene Marl. At Ashley Ferry, 10 miles northwest of Charleston, the stratum is only 1 foot thick, and rests upon Eocene Marl. The best locality is at Simmons's on Wadmalaw Sound, 10 miles from the sea-coast, as described in Tuomey's Geology of South Carolina, p. 189.

8. *The Medical Application of Electricity;* by WILLIAM F. CHANNING, M.D. Fifth enlarged edition. Boston: Thomas Hall, 158 Washington street, 1859. 12mo, pp. 242.—Dr. Channing's excellent little volume has been known for many years by its former editions. It enumerates a large number of diseases to which electricity in some form has been applied with more or less of success, with quotations of cases. In an appendix, Mr. Hall (successor to D. Davis, Jr.) gives his Illustrated Catalogue of Electromedical Instruments, so favorably known by the medical profession for their superior excellence.

9. *Treatise on the various Elements of Stability in the well-proportioned Arch, with numerous Tables of the Ultimate and Actual Thrust;* by Captain D. P. WOODBURY, U. S. Corps of Engineers. 438 pp., 8vo, with many plates and tables. New York, 1858: D. Van Nostrand.—Being No. 7 of papers on Practical Engineering published by the Engineer Department for the use of the officers of the U. S. Corps of Engineers.

10. *Maury's Sailing Directions*, Eighth edition, enlarged and improved. 383 pp. 4to, with numerous plates.

A. MALHERBE, of Metz: Monographie des Picidés, ou Histoire générale et particulière de ces oiseaux zygodactyles. This work will extend to 2 vols. fol. of text and 2 vols. fol. containing 125 colored lithographic plates and 700 figures. Only 150 copies will be published.

C. F. NAUMANN: Lehrbuch der Geognosie. Second edition, enlarged. 1st volume. xvi and 960 pp. 8vo, with 325 figures. *Leipzig*, 1858.

PROCEEDINGS BOSTON SOC. NAT. HIST., 1858.—p. 385, On the Species of Flying Fish along the coast of North America; *Weinland.*—On a new species of Skate, Goniobates, from the Sandwich Islands; *Agassiz.*—On a new species of Zeus, from Provincetown, Mass.; *Storer.*—p. 391, Notice of Dr. J. Deane; *Bouvé.*—p. 394, Fossil post-pleiocene Star fish, at Lewiston, Me.; *W. W. Baker.*—p. 395, Parasites in the American Deer; *Wyman.*—p. 396, Note on Crustacean Parasites from the Sun fish; *Kneeland.*—Feeding and Growth of the American Robin (Turdus migratorius); *Treadwell.*—p. 400, death of Dr. N. W. Cragin.

PROCEEDINGS ACAD. NAT. SCI. PHILADELPHIA, 1858.—p. 177, Description of a new Tanager, from the Isthmus of Darien, and note on Selenidera spectabilis; *John Cassin.*—p. 179, Description of a new species of Argyris; *J. C. Fisher.*—p. 180, Note on the species of Eleodes found in the United States; *J. L. LeConte.*